# THE JOURNAL OF PARAPSYCHOLOGY

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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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# *The* Journal *of* Parapsychology


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*The* Journal *of*
# Parapsychology

Volume 13 MARCH, 1949 Number 1

## EDITORIAL

### THE WASHINGTON SYMPOSIUM

It is something of an event in the history of the JOURNAL to have an entire number devoted to a public symposium on parapsychology. The symposium was held in Washington, D. C., on September 15, 1948, under the auspices of the Society for Parapsychology. For so recently formed a society this program was a highly creditable achievement. But for a field as new as parapsychology to have been able to support such a program is still more remarkable.

This series of papers and the round table discussion offer a fair measure of the progress our branch of science has been making. Members of the long-established sciences will not, of course, find it comparable to the type of program made possible in their more heavily manned and better financed fields of exploration. But if consideration is given, as it must be, to the extremely few research workers in parapsychology, this one-day review of recent developments must seem quite respectable in proportion.

No one can be expected to be as keenly aware of beginnings as the beginners themselves. To those who participate in the struggle for an opportunity to pursue their pioneer inquiries every new forward step feels large in comparison to the way it seems to those more remotely situated. Even in the detached perspective of history, however, this Washington program will hardly be overlooked as an event of significance, the first general scientific meeting of the active professional workers in parapsychology in this country.

It was only a decade prior to this symposium in Washington that the whole subject of parapsychology was brought to scientific trial at the meeting of the American Psychological Association at Columbus. At this earlier round table a determined critical attack was made on the experimental findings of the field; but this attack in September, 1938, with all its thoroughness and vigor, only served to bring to light the essential soundness of the foundations on which the principal findings were based. At the round table of ten years ago the question was that of whether ESP occurs; the round table of the Society for Parapsychology was devoted entirely to the more advanced problems of the nature of psi phenomena. The fact of their occurrence was taken as established. That represents some progress, certainly.

It is good, of course, to see such signs of progress, but better still to keep the kind of perspective of our field that will insure continuance of advance. Such symposia could contribute greatly to such perspective, and we look forward to a succession of them in the years ahead, both on a national and on an international scale.

J. B. R.

# THE 1948 SYMPOSIUM OF THE SOCIETY FOR PARAPSYCHOLOGY

## MORNING SESSION

### INTRODUCTORY REMARKS BY THE PRESIDENT OF THE SOCIETY FOR PARAPSYCHOLOGY, R. C. DARNELL

Members of the Society for Parapsychology, Distinguished Guests, Ladies and Gentlemen:

It is my pleasure and honor to welcome you here today on behalf of the Society for Parapsychology which has sponsored this first National Symposium on Parapsychology.

We would be remiss, indeed, if we did not recognize with an expression of appreciation the hearty and generous cooperation of those participating in the program, both members of the Society and others. It is very gratifying to the Society to see such fine response as is evidenced by your attendance here today.

I am sure that we will all find this meeting and the papers here presented a source of encouragement and an inspiration for future work.

May it be that this first National Symposium on Parapsychology will be but the beginning of a series of annual meetings of parapsychologists held under the same or other auspices.

I should like to call your attention to the plan of the symposium as given in the programs that have been placed in your hands. During the morning session we will hear papers that have been prepared by four active workers in the field of parapsychology. One of these will deal with the statistical tools applied in the research, and each of the other three will discuss a particular aspect of recent experimental work in which the speaker has been engaged. The afternoon session will be devoted to two main events. The first is a round table on the topic of "The Nature of Psi Processes," with Dr. J. B. Rhine as chairman. The second is an address by Dr. Gardner Murphy on the subject, "The Place of Parapsychology Among the Sciences."

# A SURVEY AND APPRAISAL OF THE STATISTICAL METHODS USED IN PARAPSYCHOLOGICAL RESEARCH

By T. N. E. Greville

I suspect that most parapsychologists regard as a necessary evil the various mathematical and statistical procedures which they are from time to time called upon to carry out in order to evaluate the results of their research. Those who hold this point of view have my entire sympathy. Their work would certainly become simpler if someone would only succeed in developing a series of "repeatable experiments" which would eliminate the need for mathematical analysis. Needless to say, this is not likely to occur in the immediate future; and, in fact, even in the so-called "exact sciences," it is frequently necessary to carry out statistical investigations in studying the causation of and the relationships between observed phenomena. However, under conditions as they actually exist, it is my conviction that the association that has grown up between mathematics and parapsychology has been mutually beneficial. For example, a recent textbook on mathematical statistics (**6**) contains a separate section on "matching theory." This useful branch of mathematical statistics, which has been the subject of several papers published in technical journals, owes its development primarily to the stimulus provided by research in parapsychology.

The benefits accruing to parapsychological research as a result of this association have been, perhaps, more apparent. The idea seems to prevail in many quarters that any scientific data, in order to be worthy of serious consideration, must be expressible in quantitative terms. While this point of view has always seemed to me somewhat exaggerated, and would, if strictly adopted, result in ignoring a substantial amount of information of great importance, it is unquestionably true that the application of quantitative methods to the various sciences has resulted, in each case, in greatly accelerating their development. Hence, the wider acceptance in scientific circles gained in recent years by the findings of parapsychological research is probably due in no small measure to the increasing emphasis on quantitative data.

In the earlier period of research in parapsychology, the principal use of mathematical procedures was in computing the probabilities of the chance occurrence of unusual observed coincidences which were suspected of being produced by mechanisms outside the system of physical, physiological, and mental processes generally recognized by modern science. The astronomical probability figures sometimes arrived at were effective in demonstrating the existence of what are now collectively termed "psi" phenomena.

More recently, there has been less emphasis on merely proving that such phenomena occur, and more attention to classifying them and discovering their relationships to other recognized phenomena. Thus, in the more recent reports, the use made of statistical procedures conforms more closely to the pattern which has become usual in the other sciences, the statistical tools most commonly employed being the familiar tests of significance based on the chi-square and *"t"* distributions.

Some of the mathematical problems that have arisen out of parapsychological research have taxed the ingenuity of mathematicians. This was true, for example, of the now famous card-matching problem. However, in many instances, such mathematical complications can be avoided, in the planning stage, by slightly altering the experimental procedure in a manner that does not seriously interfere with the purpose of the experiment. An example is the use of the so-called "open deck," in which the composition of the deck is not fixed in advance, but the card order is arrived at by some random process, such as the use of a table of random numbers, in such a way that the card in any given position is equally likely to be any one of the symbols used. When the deck is being called by a single subject only, the use of the "open deck" makes the distribution of "hits" a simple binomial distribution, which is, of course, much easier to compute probabilities for than the more complicated distributions arising from "closed" decks. Another example is the practice, in PK experiments with dice, of arranging to have each of the six faces aimed at an equal number of times, thus avoiding the necessity of making allowance for dice bias in the evaluation of results.

In seeking to apply statistical methods successfully and correctly, it is far more important to have a thorough understanding of the

logical principles of statistical inference than to be proficient in mathematics. It is not logical to apply a particular test or mathematical evaluation procedure to a given set of data unless that particular test or procedure was definitely decided upon before carrying out the experiment. An evaluation procedure which is applied as an afterthought, after examination of the data, may have great suggestive value, and may indicate profitable lines for future research, but should not be cited as evidence for any definite scientific conclusions.

For example, suppose it is suspected that PK subjects of some given type will score better in general than those of some other type. Now, suppose an experiment is actually carried out to test this theory, and a difference is obtained which would be rated as significant except for the fact that its direction is opposite to that expected. Finally, suppose the experimenter computes the probability as he would have done had the difference been in the expected direction, and then doubles it to allow for the unexpected direction of the difference. Finding that the resulting probability is still less than .01, he reports that he has obtained a significant result.

Let us imagine now that a certain laboratory in which research in parapsychology is being carried on consistently follows the practice just described, and has adopted the one per cent level of significance for all its work. Now if only chance were operating in all the experiments carried out in this laboratory, significant differences in the direction expected would occur about one per cent of the time. Differences not in the direction expected which would be judged "significant" would occur about one-half of one per cent of the time. Thus, the laboratory would obtain "significant" results about one and one-half per cent of the time, and is deluding itself in thinking that it is employing the one per cent level of significance.

Sound statistical procedure would require that this hypothetical laboratory adopt as its official policy and consistently follow one of the following four alternatives: (1) double the probability in every case, regardless of the direction of the difference, (2) ignore differences not in the direction expected, except for mentioning them as "interesting" or "suggestive," (3) continue its present practice, but consider a result as significant only if the probability obtained is

less than two-thirds of one per cent, or (4) frankly admit that it is employing a one and one-half per cent level of significance.

Currently, one of the most important and pressing problems of parapsychological research is that of finding a satisfactory method of obtaining a quantitative evaluation of material which is essentially qualitative in character, such as, for example, telepathic or clairvoyant drawings, or mediumistic utterances. Although some progress has been made, the ideal solution of this problem has not yet been found. A mathematical tool which promises to be useful in some instances, although not appearing, at first glance, to have any relation to the problem, is the theory of multiple matching. This theory has been expounded by the present author in a brief note (3) which, however, would not be intelligible to readers not versed in mathematics. It was intended to deal with card-calling experiments in which a number of subjects simultaneously call the same target, such as radio experiments in card-calling.

The possibility of applying the method to other situations was first appreciated by Pratt,[1] whose ingenious idea may be illustrated by the following purely hypothetical example. Suppose that a medium conducts a series of five sittings for five absent sitters whose identity is recorded but is unknown to the medium. The order of the five sitters is determined in a random manner. From the transcript of each sitting, a list is made of all statements of fact which are capable of verification, and each of the five sitters is asked to go over all five lists, without knowing which is the record of his own sitting, and to check all statements which correctly apply to him. In order to apply the theory of multiple matching to this situation, we may consider the "target deck" as consisting of the five sitters. The null hypothesis is that the medium was not influenced in her statements by the fact of who the sitter was at a particular sitting, and would have said the same things if the order of the sitters had been permuted. The five sittings represent five trials, and the check marks made by a particular sitter on the record of a particular sitting may be interpreted as "subjects" who choose to call that particular "target symbol" (sitter) on that trial (sitting).

Another tool of far greater usefulness is the very ingenious pref-

[1] Since this paper was presented, Pratt and Birge have explained this method in detail. See "Appraising Verbal Test Material in Parapsychology" by J. G. Pratt and William R. Birge in the *Journal of Parapsychology*, 1948, **12**, 236-56.

erential matching method developed by Stuart. This method has been fully described (4) by Stuart, and the mathematical evaluation of preferential matching data has been discussed by Greenwood (2). However, when it comes to fine-grained analysis of such data, there are some interesting mathematical problems yet to be solved. The method is of great value in making possible a valid statistical analysis of data which formerly seemed inaccessible to quantitative treatment.

A shortcoming of both the techniques just referred to is that they do not make full allowance for what Stuart (5) has called "the evident common-sense difference in value of hits among free responses." For example, the multiple matching technique does not give the medium any more credit for correctly giving the first name of a sitter than for merely stating that his first name begins with "M," provided none of the other sitters have first names starting with "M." Similarly, the preferential matching method merely records the ranks of targets and responses according to their degree of resemblance to one another, and does not give any extra credit for a spectacular hit. The only method thus far suggested which attempts to overcome this shortcoming of the other methods, is the extremely interesting one proposed by Carington (1), which, however, as Stuart has pointed out (5), "does not provide an absolutely valid statistical result."

## References

1. Carington, W. Experiments on the paranormal cognition of drawings. III. Steps in the development of a repeatable technique. *Proc. Am. Soc. psych. Res.,* 1944, **24,** 3-107.
2. Greenwood, J. A. A preferential matching problem. *Psychometrika,* 1943, **8,** 185-91.
3. Greville, T. N. E. On multiple matching with one variable deck. *Ann. math. Statist.,* 1944, **15,** 432-33.
4. Stuart, C. E. An ESP test with drawings. *J. Parapsychol.,* 1942, **6,** 20-43.
5. ———. The Carington free-drawing approach to the ESP problem. *J. Parapsychol.,* 1944, **8,** 127-38.
6. Wilks, S. S. *Mathematical Statistics.* Princeton: Princeton Univ. Press, 1943.

*Federal Security Agency*
*Washington, D. C.*

# THE MEANING OF PERFORMANCE CURVES IN ESP AND PK TEST DATA

By J. G. Pratt

Almost from the beginning of experimental work to test the ESP hypothesis, the investigators noticed that the subjects' successes from trial to trial were not randomly distributed. Among the earlier workers, Richet (15) in France, Jephson (5) in England, and Estabrooks (1) at Harvard, reported that their subjects made their highest scores at the beginning of the test.

The presence of these position effects in the data has been confirmed repeatedly in the ESP work of recent years. Rhine, in his monograph of 1934 (12), reported a decline in success through a short series of five trials quite like that found by Jephson. But for the DT or "down through" test condition with a deck of twenty-five ESP cards, his subjects gave a U-curve distribution of hits. Since that time, the U-curve in ESP runs has been reported by a number of investigators, including Sharp and Clark (19) of New York University, Pegram (7) of Guilford College, Gibson (2) who worked at Grand Rapids, Rhine (11) and Humphrey and Rhine (4) of Duke University, and Schmeidler (18) of City College of New York. Other workers have reported results from the DT condition which only partially conformed to the U-curve pattern. For example, Martin and Stribic (6) working at the University of Colorado found a *decline* in scoring throughout the run.

Frequently the similarity in the performance curves from one series to another has been remarkably close. In Figure 1, for example, we see a comparison of two curves from different series. The dotted line shows the changes in score level within the run, as found by Humphrey and Rhine. The solid line gives the same kind of information for ESP data collected by Schmeidler. In both instances the characteristic shape of the U-curve is present: high scores at the beginning of the run, scores that are near the chance average in the middle segments, and the recovery at the end to a level of scoring slightly below that of the beginning.

This general summary of position effects in the ESP data is not exhaustive. It will serve, however, to show that these lawful variations in success are both widespread and quite consistent.

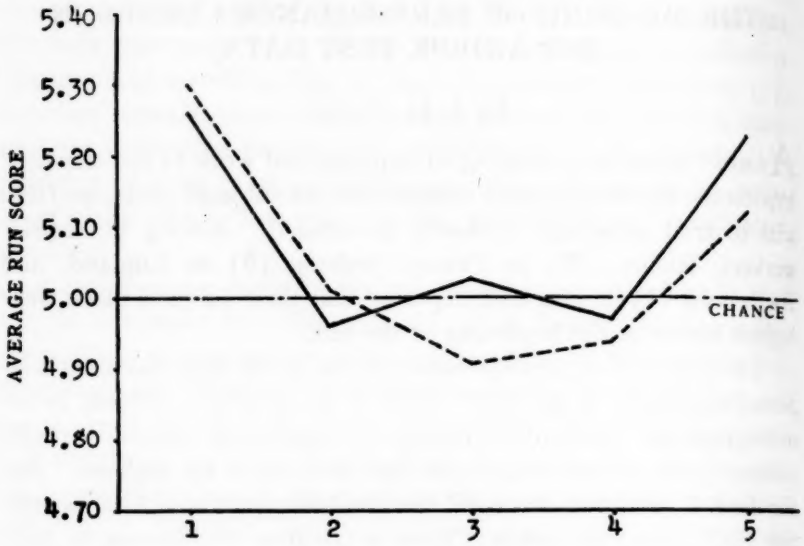


FIG. 1. Comparison of U-curves from two separate series of ESP data. The solid line gives results from Schmeidler (**18**); the broken line, from Humphrey and Rhine (**4**).

When experimental work was started to test the psychokinesis hypothesis, or the possibility of a direct mental or psychic effect upon a physical system without the use of any known physical energy or instrumentation, the investigators did not anticipate again finding position effects. Dice-throwing experiments to test the PK hypothesis were carried on for several years, and a number of independent series were completed before steps were taken to bring this work to publication. Up until that time the results were evaluated chiefly in terms of total scores. No one had concerned himself with other aspects of the data. It was known that the hits deviated significantly from chance expectation; that was all.

The PK data were subjected to more careful scrutiny in the summer of 1942. It was then observed that there were definite trends in scoring within the records. In general, the trends took two forms: there was a vertcial decline in success within the pooled column; and there was a horizontal decline from column to column across the page. At first these position effects were most important for the evidence they offered that PK had been demonstrated. This

evidence was especially strong because of the fact that no one had expected it at the time the data were collected. The occurrence of scoring trends could not, therefore, be attributed to incompent observation or recording errors, and these findings provided a control over any physical bias in the dice.

Did the differences in scoring rate within the record structure show that the subjects' concentration upon the dice had influenced the way the dice fell? To answer this question it was necessary to show that the results were beyond the range of reasonable chance variations. For this purpose a method was required which could be applied objectively and uniformly to all the series available. The many independent experiments had not been done according to a uniform pattern, and they therefore presented different structures—columns of different lengths, different numbers of columns thrown for the same target (forming a unit of data known as a "set"), and different numbers of sets on the record page. To test the statistical significance of the position effects, the investigators adopted what they called the QD analysis (**14**). This method provided a common denominator for the evaluation of all the available data. The record structure to be evaluated, either the entire page or the set, was divided into four equal quarters by vertical and horizontal center lines. This gave four groups of data in terms of the location of the trials within the unit concerned. Because of the declines both *down* and *across* the page, the greatest difference in scoring was expected on the upper-left to lower-right diagonal. Accordingly, this difference was the one always evaluated in the QD analysis. Highly significant differences were obtained, with odds against chance of the order of a hundred million to one, when the QD analysis was applied to the page. The set and the half-set, both analyzed and reported separately later, also gave highly significant results.

We have in Figure 2 a graph of the typical QD. The upper left quarter of the record structure is represented by quadrant number 1, and the lower right quarter is number 4; number 2 is the lower left, and number 3, the upper right. The graph shows the expected greatest difference in scoring between the upper left and the lower right quarters of the record structure, with the other two quadrants lying somewhere in between. This hit distribution was

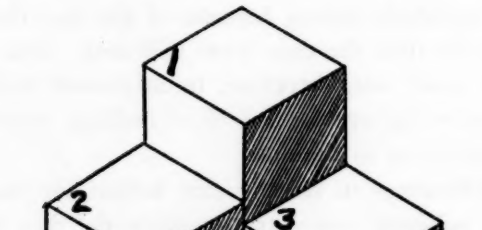


FIG. 2. A typical quarter distribution (QD) of hits as found in PK test data.

found in twelve out of eighteen separate PK series available when the analysis was first used, and some of the other six varied only slightly from this typical pattern.

However important the position effects may be as evidence of the occurrence of psychokinesis, this is not the question I wish to discuss. For the present it is enough to say that the investigators did reach positive conclusions on this issue. At this time we will be concerned with important psychological questions that arise in connection with position effects—questions bearing upon the nature of the PK effect and of psi phenomena in general. What light do the scoring trends within the record structure throw upon problems that are relevant at this more advanced stage of the research?

The analysis that is best suited to the evaluation of position effects as evidence of PK may not be the most informative one for dealing with these later, more advanced problems. The QD studies show that significant changes in scoring occurred; they do not show in detail what the scoring patterns were.

With the general objective of finding out as much as possible about the psychological factors involved, I recently analyzed some of the outstanding series in a more detailed manner. The results of a number of these studies have already appeared in the JOURNAL OF PARAPSYCHOLOGY. I wish at this time to discuss some of the psychological meanings of position effects which emerge when these and other available findings are considered as a whole.

It will only be possible to deal briefly with a few major points. The objective is to study the evidence available for valuable suggestions and to formulate fruitful working hypotheses, not to try to reach final conclusions. I shall not hesitate to indulge in speculative thinking, and I hope that there will be more in the discussion period to follow.

Perhaps the most important psychological significance of the performance curves is the indication that the PK process is a normal one. The scoring trends within the PK data show a close similarity to those already familiar from ESP results, a fact that goes far toward establishing the normality of psychokinesis within the field of parapsychology. This is one reason why some of the investigators have expressed the opinion that ESP and PK are simply dual aspects of a single basic psychic process, the function to which Dr. Thouless of Cambridge University has given the name "psi."

To the general psychologist, perhaps even more interesting than the similarity between PK and ESP is the close connection between performance curves in psi test data and in more familiar psychological observations. The position effects in ESP and PK results are clearly related to the structure of the subject's task. They lead us to think first of the principles governing the organization of psychological acts which have been emphasized by the Gestalt psychologists. The subject in the ESP or PK test definitely shows an appreciation of, a response to, his task as a structured, organized whole.

That psychological tasks or acts are usually structured by the responding organism is, of course, a generally valid principle. As such, it is not the trademark of any particular school of thought, though its significance has been more fully appreciated by some psychologists than by others. Professor William McDougall, for

example, attached great importance to this general aspect of behavior.

This organizing tendency is easily illustrated from everyday life situations. For example, the salience of the beginning and the end of the PK record column has been compared to the gardener's experience in hoeing rows of vegetables. A new row is begun with marked zest and with other evidences of strong motivation. Subjectively, progress seems rapid at the start, and the feeling of satisfaction is strong. If the rows are long, however, the sense of accomplishment fades in the workman as he approaches the middle. His interest lags. But as he gets nearer the end of the row, his motivation again becomes stronger. Effort increases, and there is a return of the feeling of satisfaction which he had at the beginning.

The same general pattern of response might be illustrated by our experience in memorizing a poem or in listening repeatedly to a musical selection. We tend to learn first those items that come at the beginning, and next in order, we become familiar with the closing words or notes.

When we find this fundamental characteristic of normal behavior in ESP and PK data, we have ample reason for inferring that we are dealing here with established psychological principles showing themselves in only less familiar forms.

But the clearest evidence of all that PK and ESP are closely related to more familiar psychological functions comes from the experimental studies of learning and memory, particularly from quantitative experiments on the learning of rote series. This is not surprising, for the usual ESP or PK test situation—essentially one in which the subject goes rapidly through a series of similar responses—is in many respects like that found in such learning experiments. We have, therefore, a special interest in asking how performance compares in learning experiments and in PK and ESP tests.

In Figure 3 we have a comparison between learning nonsense syllables and performance in a PK test. The learning was measured by the success of recall after one presentation of the series. The graph in the upper left-hand part of the figure shows the decline in efficiency of recall as it occurred in the second through the last positions of a series of six syllables (**16**). Recall was not tested on the first syllable; it was shown to the subject as the cue to try to

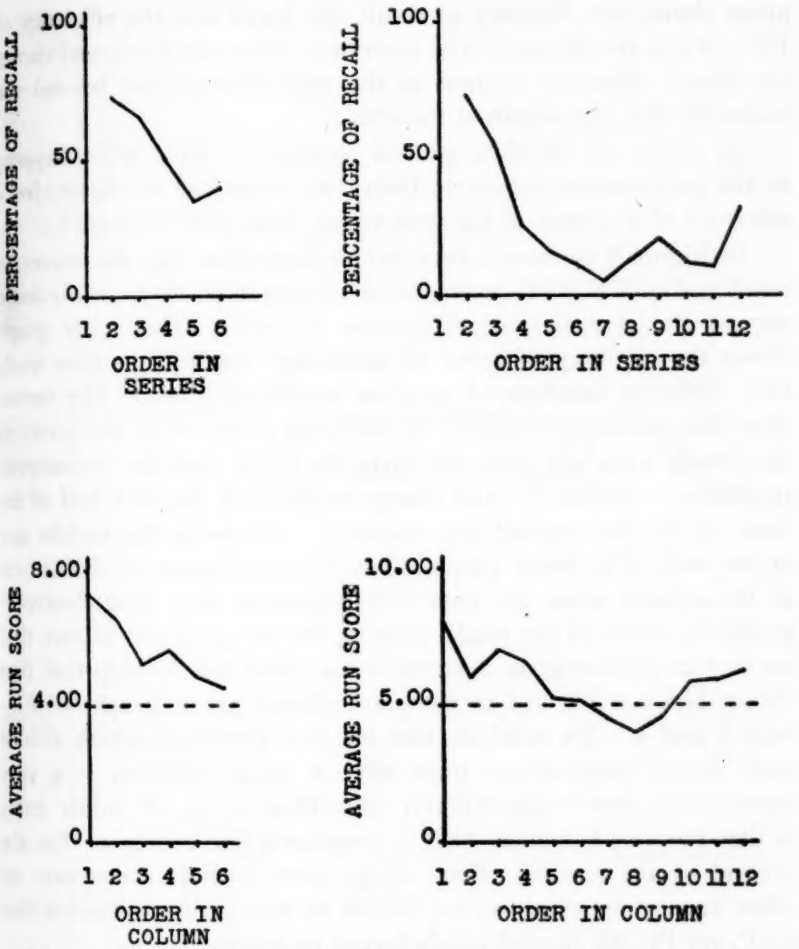


FIG. 3. Comparison of success in learning nonsense syllables and in PK performance in relation to the number of trials. The upper graphs show learning (as measured by recall) in terms of the trial position in a short (six-trial) and in a long (twelve-trial) series (**16**). The lower graphs make the same kind of comparison for PK results (**8, 10**).

anticipate the presentation of the second one. In the lower left-hand graph we see the decline in success shown in a subject's PK performance in a six-trial sequence of throwing dice for a specified target face (8). The similarity between the two cases speaks for itself. On the right-hand side of Figure 3 are two examples of performance curves based on twelve-place series. The upper graph

again shows the efficiency of recall, the lower one the efficiency of PK (10) in the different trial positions. The two U-shaped curves are almost identical: highest at the start, lowest just beyond the midpoint, and high again at the end.

So much for the close general similarity. Now what happens to the performance curves in these two situations as the subject's efficiency of response in his task varies from poor to good?

In Figure 4 we have a very strong suggestion that the curves of recall and of PK performance even *change their shape in the same way* as the general level of success increases. The upper graph shows the efficiency of recall of three-digit numbers in tests made after different numbers of practice repetitions (16). The curves show the percentage recalled for different positions in the series in tests made after the first, the fifth, the ninth, and the seventeenth repetitions. Relatively little change occurred in the first half of the four curves, but marked improvement is shown in the middle and at the end. The lower graph shows the distribution of PK scores in the column when the data were separated into four "success" groups in terms of the total scores on the set (10). It is rare that we find in psychological observations a closer relationship that that shown between PK and memory as reflected in the graphs of Figures 3 and 4. To conclude that the two processes which yielded such similar observations must have a lot in common is a very conservative hypothesis to derive from these facts. A bolder guess is that the two functions have a common psychic origin. On the ground of the position effects alone, then, even if there were no other supporting evidence, we should be led to the conclusion that ESP and PK are normal psychological processes.

A second principle which we may derive from the scoring trends within ESP and PK data is this: the process which produces the position effects is a spontaneous or non-rational one. The first trial of the task is the one which is most likely to stand out. This is the point at which the subject is most nearly free from rational factors in making his response. He is at this point in a dual role as it were: on the one hand, he is an observer, waiting to see what will happen; at the same time, he is a strongly motivated and eager participant in the test. The special blend or quality of motivation resulting seems to me to be one of the conditions necessary for the

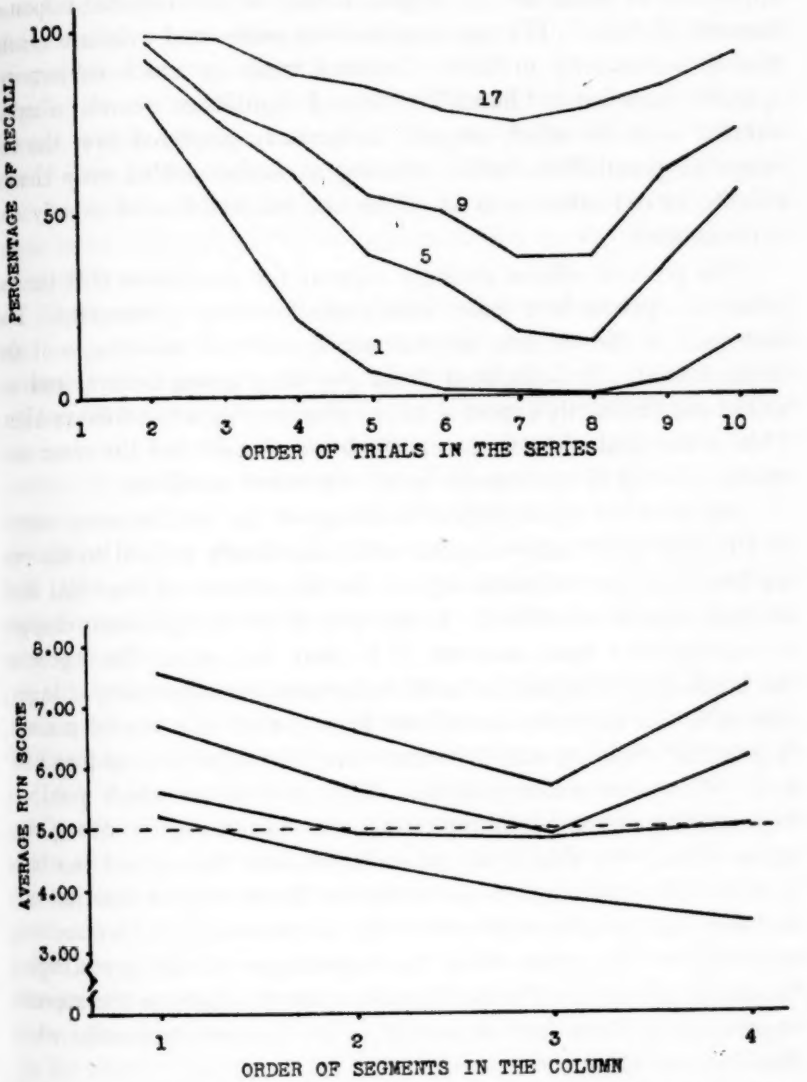


FIG. 4. Comparison of effect of number of repetitions upon distribution of success in learning with effect of total score upon distribution of success in PK performance. The upper graph shows the change in the distribution of success of recall in a series of ten three-digit numbers with the number of practice repetitions indicated (**16**). The lower graph shows the distribution of hits in a twelve-trial PK column in terms of four scoring levels (**10**).

occurrence of what we call a spontaneous or non-rational response. Recently Scherer (17) reported some experimental evidence on the role of spontaneity in ESP. Isolated trials in which subjects responded only on a "hunch" produced significant scores, whereas isolated trials in which subjects deliberately pondered over the full range of possibilities before making a choice yielded only chance results, as did other tests in which the trials followed rapidly one upon another.

The position effects strongly support the conclusion that the psi processes operate best under conditions favoring spontaneity. The first trial of the set has, psychologically, all the advantages of the isolated trial. It *is* isolated from anything going before, and we would not ordinarily expect it to be influenced by what follows after. This is the trial, therefore, on which we should find the most successful scoring if spontaneity is an important condition.

Still another psychological meaning of the performance curves in PK and ESP data—a point which is closely related to the one we have just been considering—is the importance of the *right kind* of motivational conditions. In any test in which significant changes in scoring level have occurred, it is clear that either the degree or the kind of motivation, or both, underwent corresponding changes; otherwise the responses would not have varied in a lawful manner. A practical meaning which we can draw from the facts is that ESP and PK are extremely elusive. The motivation which produces high scoring at the beginning of the task is not merely strong conscious effort, for this is an experience which the subject can have at will. The truth is that we do not yet know how to describe with certainty the peculiar motivation that is necessary, and we are only beginning to appreciate fully the importance of the psychological factors in psi tests. The performance curves illustrate the supreme importance of these factors, as well as the extreme elusiveness which characterizes them.

One of the main emphases in parapsychology today is upon the study of psi abilities in relation to personality characteristics. If any evidence were needed that personality factors are important to a fuller understanding of ESP and PK, we could make out a good case for this on the basis of individual differences in performance curves alone. Perhaps in this instance the ESP results are more

revealing than those from PK. Standard record sheets and uniform test procedures have been used more commonly in the former, and comparisons among subjects may be made with more assurance. The ESP results show that many different individual patterns of success within the run have been observed. In DT, for example, the U-curve is the most common distribution, but it is not the only one on record. Both declines and inclines have occurred, and in some cases successful DT scoring has shown no significant internal score differences.

Even in the PK data there is evidence that the manner in which the hits are distributed is often an individual matter. We might compare, for example, the distribution in the first column of the set for the two major subjects in the Gibson Machine Series. Both of these subjects achieved a significant total score in their witnessed work of this series. The work of L.H.G. is characterized by marked beginning and ending salience in the first column of the set (9) but that of E.P.G. done under the same objective test conditions does not follow this pattern. On the other hand, an individual subject may show strikingly similar patterns of scoring in different analyses of test results.

We have such a comparison between the vertical and horizontal distributions within two pooled sets by a single subject in Figure 5. In this experiment the subject was throwing six white dice and six red dice. She was trying to get a high score on ones with one set of the dice (high aim) at the same time that she was trying to get a low score on ones with the other set of dice (low aim). The two curves on the left show the horizontal and vertical distributions of scores on the high-aim trials. The two curves on the right show the same two distributions of scores for low aim. The objective of the experiment was to see if the subject could go both high and low at the same time (3). Our interest here, however, is centered on the close similarity in hit distributions when measured *vertically* and *horizontally* within each set. The patterns are so nearly identical as to suggest that they reflect an individual characteristic of this particular subject.

Parapsychologists have had a great deal to say recently about psi as a subconscious mental process. The subject who is demonstrating his ESP or his PK ability does not know which trials are

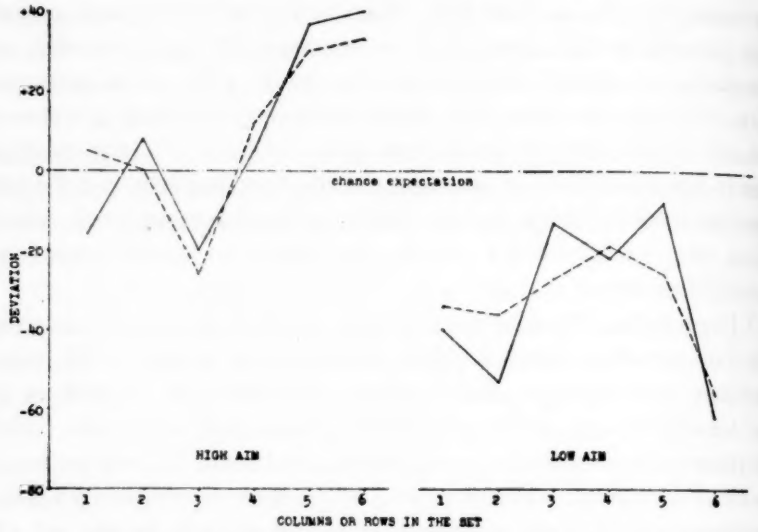


FIG. 5. Similarity between vertical (broken line) and horizontal (solid line) distributions of hits within the PK records of an individual subject (**3**).

successful and which are not. If we had no other ground for this important conclusion, we could well infer it from the performance curves. In many cases the most successful scoring has been phenomenally good, and the subject has passed within a matter of seconds from these outstanding trials to others in which he performed at the chance level. If ESP or PK generally were accompanied by any identifying conscious experience, we should certainly expect that a subject would soon learn to know when he was successful. Yet this has never happened; the position effects have always been found only when the data were analyzed at the end of the series.

Thus far we have been concerned with some specific suggestions arising out of performance curves in ESP and PK test data. It might be worth while to generalize to see if we can reach any sort of tentative conclusions on the psychological explanation of these effects. Rhine, in presenting the results of an extensive series of investigations of terminal salience in ESP (**13**), discussed his findings in terms of conflicting lines of rational association. According to this view, the low scoring in the middle of the run or segment results from the fact that the subject becomes increasingly entangled

by associative tendencies which are evoked by his calls as he takes the test, Though weak in themselves, these conflicting rational tendencies are strong enough to inhibit the subtle and elusive psi response. On the other hand, Schmeidler (18) seemed to favor the view that the explanation is to be found in terms of changes in the subject's motivation in relation to the beginning, middle, and end of his task. At first glance this looks like an interpretation which is opposed to the one favored by Rhine.

I should like to suggest that the difference between these views is more apparent than real. I think that the position effects can be understood only in terms of both motivation and the cognitive factors by which the subject directs his response. Thouless (20) has referred to the important problem of how the subject directs his psi abilities upon the target, a process which he has called *singularization.* The performance curves may afford the best opportunity for studying this aspect of the operation of psi processes. Salience of certain trial positions may be due to differences in the degree of success at singularizing the target. In other words, we may be faced not only with a question of getting the right kind and degree of motivation, but also of the control and channeling of this motivation to produce the desired response.

Finally, we come to the question of what practical bearing the performance curves should have upon future ESP and PK research. The evidence, particularly that from the PK work, strongly indicates that it is important to design the tests to take advantage of high scoring on salient trials. It behooves anyone planning to do a PK experiment to consider carefully whether he has the most favorable design possible in the light of present knowledge. If his tests are long, monotonous sequences of throws, he runs a grave risk that even the most gifted subject will have his successful scoring on a small percentage of trials swamped by low scoring on the rest. The immediate practical objective should be to design tests which offer the subject as many favorable trial positions as possible. This means reasonably short columns and few columns to the set, perhaps only one or two. Care must be used to make the pause or break between sets psychologically real so that the start on the next sequence of throws will represent a true beginning point. I am, of course, describing a test in which it is possible to obtain a number of trials by

a subject at a single session, thus keeping the advantage of accumulating data more rapidly. Aside from this factor, the most favorable test might be one in which subjects are allowed only one throw at a time. These considerations raise questions which are worthy of, and badly need, experimental answers.

## References


1. Estabrooks, G. H. A contribution to experimental telepathy. *Bull. Boston Soc. psych. Res.*, No. 5, 1927.
2. Gibson, E. P. A study of comparative performance in several ESP procedures. *J. Parapsychol.*, 1937, **1,** 264-75.
3. Humphrey, B. M. Simultaneous high and low aim in PK tests. *J. Parapsychol.*, 1947, **11,** 160-74.
4. Humphrey, B. M., and Rhine, J. B. A confirmatory study of salience in precognition tests. *J. Parapsychol.*, 1942, **6,** 190-219.
5. Jephson, Ina. Evidence for clairvoyance in card-guessing. *Proc. Soc. psych. Res.*, London, 1929, **38,** 223-68.
6. Martin, D. R., and Stribic, F. P. Studies in extrasensory perception. III. A review of all University of Colorado experiments. *J. Parapsychol.*, 1940, **4,** 159-248.
7. Pegram, M. H. Some psychological relations of extrasensory perception. *J. Parapsychol.*, 1937, **1,** 191-205.
8. Pratt, J. G. Lawfulness of position effects in the Gibson Cup PK Series. *J. Parapsychol.*, 1946, **10,** 243-68.
9. ———. Restricted areas of success in PK tests. *J. Parapsychol.*, 1947, **11,** 191-207.
10. ———. Rhythms of success in PK test data. *J. Parapsychol.*, 1947, **11,** 90-110.
11. Rhine, J. B. Evidence of precognition in the covariation of salience ratios. *J. Parapsychol.*, 1942, **6,** 111-43.
12. ———. *Extrasensory Perception.* Boston: Bruce Humphries, 1934.
13. ———. Terminal salience in ESP performance. *J. Parapsychol.*, 1941, **5,** 183-244.
14. Rhine, J. B., and Humphrey, B. M. The PK effect: special evidence from hit patterns. I. Quarter distributions of the page. *J. Parapsychol.*, 1944, **8,** 18-60.
15. Richet, C. *Thirty Years of Psychical Research.* New York: The Macmillan Co., 1923.
16. Robinson, E. S., and Brown, M. A. Effect of serial position upon memorization. *Amer. J. Psychol.*, 1926, **37,** 538-52.

17. SCHERER, W. B. Spontaneity as a factor in ESP. *J. Parapsychol.*, 1948, **12,** 126-47.
18. SCHMEIDLER, G. R. Position effects as psychological phenomena. *J. Parapsychol.*, 1944, **8,** 110-23.
19. SHARP, V., AND CLARK, C. C. Group tests for extrasensory perception. *J. Parapsychol.*, 1937, **1,** 123-42.
20. THOULESS, R. H. A program of parapsychology. *J. Parapsychol.*, 1948, **12,** 113-18.

*Parapsychology Laboratory*
*Duke University*
*Durham, North Carolina*

## PERSONALITY CORRELATES OF ESP AS SHOWN BY RORSCHACH STUDIES

By GERTRUDE R. SCHMEIDLER

AS I PREPARED to write this paper, it seemed to break into two parts. The first and longer one is a factual report of my research in ESP. It will contain little that is new, but it is good to be able to tell you that the current data are consistent with the earlier ones. The second part will try to take a broader view of the whole research, to evaluate it and its limitations, and to speculate on how far we have a right to hope that it can be repeated in other laboratories.

The onset of the work, as some of you know, came when I found that the ESP subjects who laughed at the concept of telepathy and disapproved of doing research on it, made low ESP scores; and the subjects who were interested did better. I framed an experiment to separate these two groups by the simple expedient of asking them at the beginning of the session what they thought about the problem. On the basis of their answers, before they made their first guess, they were divided into two classes, which I called for brevity the sheep and the goats. The sheep were the subjects who thought that paranormal success in the experiment was at least a possibility; the goats denied any possibility of paranormal success under the conditions of the experiment. The prediction about the data was that the average scores of the sheep would be above chance expectancy; and that the goat averages would be at chance or below.

(Now, on the basis of experience, I should prefer to state the relationship between them in more general terms, and to predict only that the sheep average should, given enough cases, be significantly higher than the average of the goats.)

All the customary experimental precautions were taken. Targets were prepared by an assistant from a table of random numbers, so that no tricks of logic or memory could help the subject guess correctly. A permanent record was made of the targets before the experiments began. They were concealed both from the subject and from me, so that I could not inadvertently hint what the first symbols in the list would be. A permanent record of the responses was made by the subject. Either two or three independent scorings were made of the hits.

The results (Figure 1) were in line with the prediction: in nine

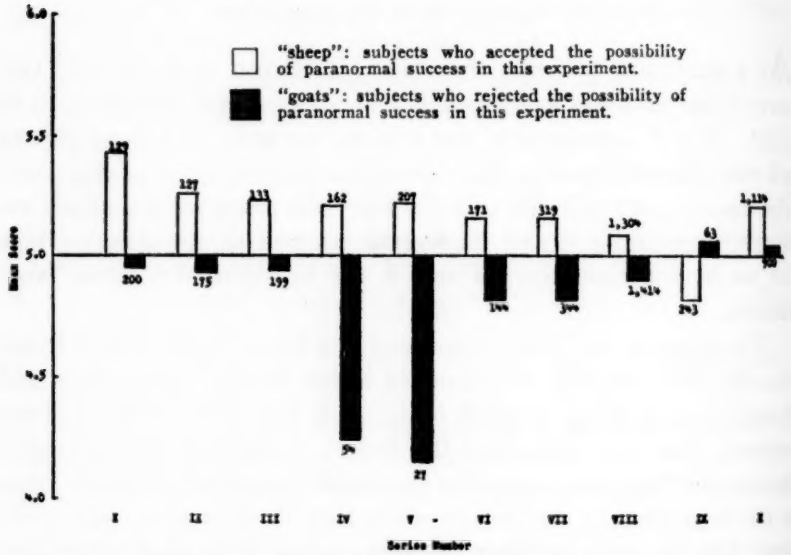


FIG. 1. Comparison of scores obtained in ESP experiments by subjects who either accepted the possibility of paranormal success in the experiment or rejected this possibility. The number of runs in each series is shown above or below each bar.

out of ten independent series the sheep average was higher than the goat average. In all nine cases the sheep average was above chance; in eight cases the goat average was below chance. When all the scores collected in this research are summed, they are highly

significant. The difference in scoring level between the sheep and the goats would be expected to occur by chance in only one experiment out of three and a half million. The probability for the sheep scores is about one in two million; and even the goat scores can be considered significant, with only about one chance in a hundred of their falling so low by chance alone.

The actual difference between the sheep and the goats is pitifully small. A score of 5 would of course be expected by chance; 3,909 sheep runs have a mean score of only 5.15; and the mean score of the 3,520 goat runs is 4.92. The difference between them, I have calculated roughly, is of the same order of magnitude as the house percentage in a roulette game. And I think there is an analogy here: no one can expect to run an honest roulette wheel at a profit unless he has a large reserve of funds; and probably no experimenter could expect significant differences between groups of subjects, using the sheep-goat criterion alone, unless he has a large enough reserve of patience to collect a very great many cases.

The differences, small as they are, cannot be dismissed as accidental. They can, I think, be reasonably interpreted in only one way: they show that ESP occurs and that it is influenced by the attitude of the subject toward the experiment. The sheep will, in general, hope to do well; and in the event, they did well. The attitude of the goats is likely to be more complex. We can imagine the typical goat as a friendly person, who is willing to act as a subject even in what he privately considers to be an exceptionally foolish experiment. He is consciously trying to cooperate; in response to a questionnaire a goat usually reports that he is interested in the experiment and is trying to do well. But our presumption is that his disapproval at some unconscious level affects whatever processes make for good ESP scores, and that he behaves as he really feels—negativistically, scoring low, rather than cooperatively, as he thinks he feels.

If this seems too fine-drawn and psychological to some of you, let me try to strengthen the argument for unconscious negativism influencing behavior by examples from other areas. I suggest that there is likely to be a lot of "accidental" breaking of dishes in a kitchen where the dishwasher urgently wishes that she were doing something else; or that the secretary who is dissatisfied with her po-

sition but not quite ready to resign is likely to make an extraordinary number of "careless" mistakes while she is still nursing her grievances. If you were to ask the secretary whether she makes the mistakes deliberately, or if you asked the low-scoring goat in these experiments whether he tries to miss the target, you would in both cases hear a perfectly sincere denial.

Perhaps it is underlining the obvious to stress that the subjects' unconscious rather than their conscious motivation makes the goat scores significantly different from the sheep scores. But the point is important in planning research, for it implies that we cannot get at the psychological determinants which inhibit or misdirect ESP by the simple method of asking about them directly. The subjects do not know the answers about their own inner states.

It is reasonable at this point to turn to the psychological tests that have been devised to help solve just such problems. One of the most widely used, and in many ways one of the best, of these tests is the Rorschach. It was developed originally to diagnose psychosis or neurosis, but we know now that it shows so much about a person's typical modes of response that it can be useful with normal individuals for vocational counseling, guidance, or, of course, research. I have in the last three years scored the Rorschachs of more than 650 individuals who took part in the experiments just described and have found certain patterns in their Rorschach records which indicate whether my subjects are likely to score well or badly at ESP.

The first lead came from experiments performed by Dr. Ruth Munroe at Sarah Lawrence College. She gave Rorschachs to the freshmen of several entering classes and determined from each record a figure which served as an index to the girl's general social adjustment. College grades could be predicted from this index of adjustment status about as accurately as from the results of intelligence tests: the girls with best adjustment made the highest grades, and the girls with poorest adjustment were the most likely to flunk. (Intelligence test and adjustment scores in combination gave, of course, a much more accurate prediction than did either alone.) The results indicate that the well-adjusted students could make profitable use of their intellectual abilities, but that the girls with many personal difficulties were likely to be too concerned with their

own problems to react in the expected, adaptive way in a college setting.

Applying Dr. Munroe's criteria to the Rorschachs of the ESP subjects, it seemed that the same principle was at work. The well-adjusted sheep in a preliminary sample made higher scores than the poorly adjusted sheep; and the well-adjusted goats made lower ESP scores than the poorly adjusted goats. After these subjects had been tested and the results had been stated, the records shown in Figure 2

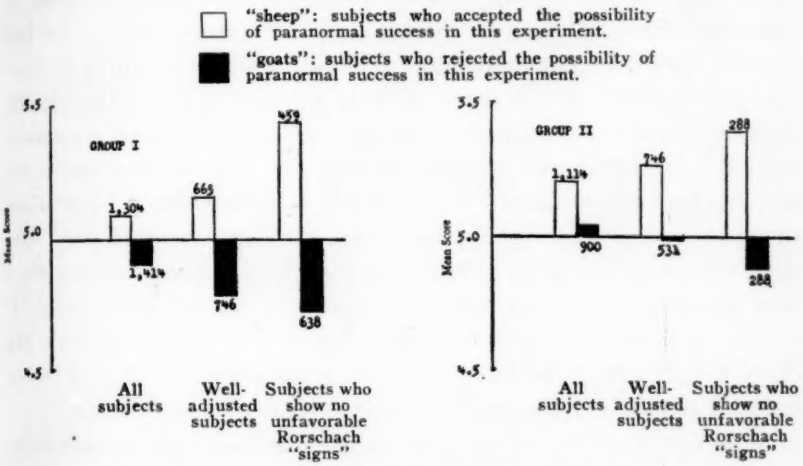


FIG. 2. Comparison of scores obtained in an ESP experiment by two unselected groups of subjects; by the members of each group who are well-adjusted; and by the members of each group whose Rorschachs are free of seven unfavorable "signs." Numbers above and below the bars show the number of runs.

were obtained. We can therefore take both the groups of Figure 2 as a check on the hypothesis that, all other things being equal, well-adjusted subjects will do better at ESP than will poorly adjusted subjects.

The results are consistent with the theory. All sheep with Rorschachs tested in this clairvoyance-type experiment after the adjustment hypothesis had been stated, had a mean score of 5.12; the well-adjusted sheep had a mean of 5.18. All goats with Rorschachs had a mean score of 4.96; the well-adjusted goats had a mean of 4.87. The difference between well-adjusted sheep and goats would be expected to occur by chance only about four times in a hundred thousand similar experiments, but the difference between

poorly adjusted sheep and goats is negligible. We can conclude that the well-adjusted subjects are more likely to score as they really want (that is, the sheep will score above chance and the goats will score below chance); but that ESP scores of subjects with poor adjustment are harder to predict.

It would be a misuse of the rich material that the Rorschach offers if we looked no further than for this general statement of adjustment. I have accordingly been tabulating about thirty other Rorschach scores, hoping to find a relationship between some of them and the ESP scores. Seven separate factors seem to be important, which I shall first describe in technical terms for any Rorschach students that are here, and then translate into English for the non-Rorschach students. They are: a large number of responses, high F%, rigid human movement, 60% or more of movement responses, no indication of either color or shading shock even when shock is scored very lightly, too much CF, or too much C. Any one of these served as an indicator of poor ESP scores. (Recently I have also come to believe that coartation is associated with low ESP scores for both sheep and goats and that the dilated ambiequals are likely to make high scores, but this has not been tested and is not included among the factors used here for prediction.)

Taken out of Rorschach jargon, this list means, approximately, that my subjects did not do well in ESP experiments if their Rorschachs showed them to be strongly inhibited or constricted, unable or unwilling to respond freely to a situation; nor if they pushed themselves too hard intellectually; nor if they often acted overemotionally.

My procedure in the research has been to administer the ESP tests as described earlier and to complete the scoring of the Rorschachs before seeing the subjects' ESP scores. With the first group shown in Figure 2, I worked over the Rorschach tabulations with the ESP scores in front of me, pulled out all subjects who showed any one of the seven "signs" just listed, and found that the remaining sheep had comparatively high averages (their mean was 5.44) and the remaining goats had much lower averages (their mean was 4.84). All subsequent subjects, listed as the second group of Figure 2, were used to check the finding. The results in the check group were similar: the sheep who showed none of the signs had a mean

score of 5.40 and the goats had a mean score of 4.88. The difference between sheep and goats would be expected to occur by chance less than three times in a thousand such experiments. The sheep who showed one or more of the signs had a mean of 5.14; the goats who showed one or more of the signs had a mean of 5.13; and the difference between them is of course negligible.

All this looks very nice. The check group of subjects were as much like the preliminary group as anyone could expect; the findings are significant; and it would be pleasant to stop this talk now, with the conclusion that the seven Rorschach criteria just listed are, individually, diagnostic of poor ESP scores. But I cannot feel secure in such a conclusion, chiefly because of an experiment performed about two years ago, which I should like to describe to you. This was the exceptional group that you may remember from Figure 1, where the sheep had lower scores than the goats. There were comparatively few subjects; the results were never repeated; and still it seems to me that they showed so much internal consistency that they should not be disregarded. Everything that I say about them has to be considered mere speculation, based on an inadequate number of cases; but it is a speculation that it seems to me to be necessary to make.

On the surface the group did not seem unusual: they were members of the psychology classes of Dr. Burke Smith, who loaned them to me for an experiment during a visit I paid to Duke. But I created an unfortunate atmosphere in the class: my manner was hurried in an effort to crowd as much as possible into the experimental period, and it was probably tense because of the effort of making my voice carry in a large room. Any impressions of pressure and tension were undoubtedly exaggerated, partly by my Northern way of speech, and particularly by contrast with the classes' usual instructor, who has a warm, relaxed classroom manner and a pleasant, easy-going sense of humor. The group probably thought of me as a pedantic Northerner, with whom most of them could feel no strong desire to cooperate.

The results shape up as if the subjects felt that way. The sheep scored below chance, with a mean of 4.82; the mean of the goats was 5.06. The well-adjusted sheep had an average of 4.81; the well-adjusted goats had an average of 4.56. This looks, if my

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previous hypotheses are sound, as if the unconscious motivation of the sheep led them to make low scores, and as if the motivation of the goats was similar or even stronger. Of the handful of subjects who showed none of the unfavorable Rorschach signs, the sheep had a mean score of 4.65; and the one goat had a mean of 4.22. Reexamining the data to find which subjects did well, it was interesting to see that the sheep who made high ESP scores were men who, according to their Rorschachs, tended to be reserved, pedantic and unresponsive.

By statistical criteria, this group seems to be drawn from a different population from that of the other subjects. My feeling about it is that their attitude was indeed different from the attitude of the other groups; and that this factor strongly influenced the personality dynamics that make for successful scoring. I should like to generalize from the results in some such way as this: in a group which considered the atmosphere of the experiment to be unpleasantly cold and intellectual, only the subjects who were themselves rather cold and intellectual responded positively and made good scores. In the other experiments a different atmosphere was established, and a different personality pattern in the subjects led to successful responses.

If this generalization is correct, what are its implications? One conclusion would be that my research does not show the personality correlates of ESP ability as such, but only of ESP ability under the particular conditions of the experiment. Whenever the situation varies widely from these conditions, we can expect the optimum personality pattern to vary also. (And let me state explicitly here that the crucial aspect of the situation is almost surely not a matter of the "objective" conditions but of the subjective ones: how the subjects feel about the experiment.)

A practical implication, following the same line of reasoning, is that anyone who tries to repeat the experiments must be willing to take a large enough number of cases so that he can go through the same procedure. He should not necessarily expect to find that the personality traits which were associated with poor scores in my research will also be associated with poor scores in his own. He should rather examine his records to find the pattern which seems to differentiate good scores from bad, and then repeat the experiments

under as uniform conditions as possible (that is, trying to build up the same kind of rapport that he had previously) to see if the same personality syndrome will again differentiate the poor and the good scores. I should expect him to find that this syndrome was different from the one described here if his subjects were more intensely interested or more enthusiastic than mine had been; or if he, whether deliberately or unconsciously, made them feel bored with the experiment or hostile to it. The most we could hope, then, is that his own results should be consistent from one large series to another.

This is a disappointingly limited conclusion. But if it is true, it may at least have the negative virtue that it will keep us from looking for uniformities where none should be expected, and thus encourage us to look deeper or in a different direction for the factors that are basic to ESP success.

*Department of Psychology*
*College of the City of New York*
*New York, N. Y.*

## THE RELATION OF ESP TO MODE OF DRAWING

By Betty M. Humphrey

Now that the existence of ESP and PK has been established, parapsychologists have in recent years turned their attention primarily toward the problem of finding out more about the nature of these psi phenomena. One way of learning more about psi and of increasing our control over its production is to find out more about the subjects in our experiments. For example, we want to find out whether there are certain personality characteristics which will discriminate between the high- and low-scorers in ESP tests. If there are certain personality traits or combinations of traits which distinguish the high-scoring subjects from the low-scoring ones, we should be able to predict success or failure in our ESP tests and thus gain increased control over the phenomenon.

In present-day studies of personality, projective techniques are being used widely. In a projective test the subject is given a relatively unstructured situation to which he is required to respond, and in so doing he reveals certain characteristic ways of seeing life and of reacting to it emotionally. Thus we find out about the

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individual's private world because he has to organize the material and interpret it and react to it in his customary manner. Free drawings, for example, reveal a great deal about the person who has drawn them. In recent years, a number of studies have been made showing the differences between drawings made by adjusted and maladjusted individuals. Some of these studies have dealt with the analysis of the *form* quality of the drawing—that is, *how* it was drawn rather than *what* was drawn. The expressive movements employed in drawing leave tell-tale clues which enable the investigator to tell, for instance, whether the artist worked tensely and rigidly or freely and uninhibitedly.

The methods of analyzing form qualities in drawings have proved quite helpful in the study of the personality characteristics of good and poor subjects in ESP tests. A great deal of experimentation in ESP has involved tests in which the subjects are asked to make drawings in the attempt to reproduce or identify concealed pictures, known as target pictures. For example, in clairvoyance tests, the subject is given a sealed opaque envelope and asked to draw his impressions in regard to the picture contained in the envelope. In tests of general ESP, which most subjects interpret as being telepathic tests, an agent looks at a target picture in a distant room while the subject draws his impressions. The drawings obtained in these ESP tests would be quite amenable to the same type of analysis in terms of form quality as clinical psychologists are using in evaluating free drawings. The ESP test in itself constitutes a type of projective technique.

Before going into detail about the way in which the ESP drawings were analyzed for personality differences, I should like to describe briefly how drawings experiments are carried out and how the drawings are scored for evidence of ESP. In the attempt to learn more about how ESP functions, my colleague, the late Dr. C. E. Stuart, developed a drawings technique (8) which he called the "free response method." He considered the possibility that ESP may not be an all-or-none affair—that the subject may not give just a simple hit or a miss, but that he may unconsciously perceive by ESP certain elements of a target picture and then distort them in characteristic ways, so that his response drawing is not a good reproduction of the target drawing. The subject might, for

instance, respond with an association which for him is peculiarly linked with the theme of the target picture. Thus, the ESP drawings test becomes a miniature real-life situation in which the subject may respond in his own characteristic manner.

In a typical drawings experiment, the subject is asked to make drawings in response to four different target pictures. After the test is finished, the experimenter shuffles these pictures and the response drawings so that they are not in their correct order, and then gives this material to an independent judge to match. The matching method used is known as "preferential matching." This procedure is too complex to describe in detail here and it has been fully presented in published reports (**3, 8**). By means of this method, the score expected by chance alone is easily computed, and conventional statistical procedures are used to determine the significance of any deviations from the "chance" score. For the benefit of the statistical minded, I shall say parenthetically that the test of significance used in all drawings experiments discussed here was "Student's" *t*-test, and the probabilities were obtained from "Student's" *t* distribution.

Now we are ready to see how these ESP drawings were analyzed for differences in *form* qualities. Dr. Stuart had carried out a number of ESP experiments using the procedure just described. Concerning the results of these tests, I knew nothing. I took all of his response drawings, and, without knowledge of the ESP scores obtained, I rated them in terms of one particular form quality known as "expansion-compression." This expansion-compression criterion was defined by Dr. Paula Elkisch in her study of children's drawings (**2**).

*Expansion* may be defined as that quality in a drawing which implies that the artist has expressed himself freely and uninhibitedly —that he has been able to draw with freedom of expression, imagination, and a sense of proper relationship between the objects drawn and the background. Or, briefly, an expansive drawing is one that shows imagination and is made with free, bold lines and good use of the drawing space available.

*Compression,* however, is that quality in a drawing which indicates that the subject has been unable or unwilling to express himself freely. Some of the signs of compression are timid, faint lines;

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rigid, minute pictures; or conventional and abstract pictures. Another common sign of compression is poor proportion; the compressive drawings are usually too small for the space available. Paucity of ideas is another sign indicative of compression. If a drawing gives one the feeling that the subject has a conception of the world as being hostile, it is also rated as compressive.

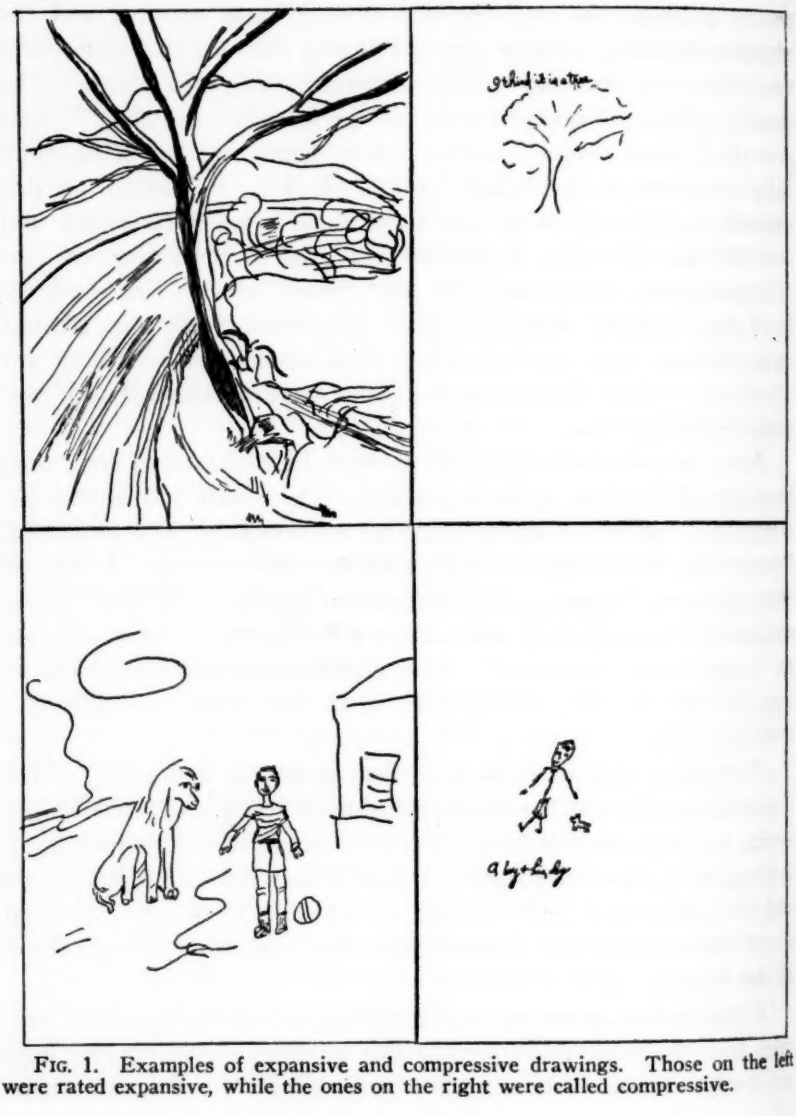

FIG. 1. Examples of expansive and compressive drawings. Those on the left were rated expansive, while the ones on the right were called compressive.

The concept of expansion-compression may perhaps be more easily understood from a few examples taken from some of the ESP series. In Figure 1, the first drawing of a tree was rated expansive, for it shows that the subject expressed his ideas freely and made good use of the drawing space available. The second drawing of a tree, however, serves as a typical example of a compressive response. The small tree was drawn very faintly and was compressed into the upper left corner of the page. These features of the drawing indicate that the subject was inhibited in expressing himself—he gave a faint and unimaginative representation of his idea. Furthermore, the label "I think it is a tree" serves to reaffirm indications from the drawing that the subject felt timid and uncertain. Unnecessary labeling of pictures is often found in compressive drawings.

The subject who made the expansive drawing of the tree obviously possessed artistic skill, and one would hardly expect such a subject to feel inhibited in performing a task at which he excels. However, expansion is not synonymous with drawing skill. The third picture in Figure 1 shows an expansive drawing made by a subject who was not artistically gifted. Yet his drawing of a boy and a dog does show some imagination and good use of the drawing space, even though the details are rather crudely executed. At least one can say that there is little evidence of compression in this picture. In contrast with this picture, however, is the fourth drawing in Figure 1, which deals with the same subject matter as did the third. Here, though, the boy and the dog are faintly drawn, taking up only a small part of the page. This drawing was rated as compressive. Again the unnecessary label confirms one's impressions of insecurity.

The examples given show extremes whereby the concepts may be more easily understood. In many cases, it is difficult to make a decision and in such cases the drawings are given a rating of "undecided."

In applying the expansion-compression criterion to the drawings obtained by Dr. Stuart in his ESP experiments, I considered each drawing in the light of the criterion and assigned to each a score indicative of the type of form quality present. Thus, on the basis of the four drawings made by each subject, it was possible to give

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an expansive or compressive rating for the entire set of drawings. After I had rated all of the drawings from a given series and had divided the subjects into two groups, expansives and compressives, I took the ratings to Dr. Stuart, and together we added the scores for all the expansive subjects and obtained an average score for the expansive group. In a similar way the score was found for the compressive group. The first experiments considered in this way were four clairvoyance series which had been conducted at Stanford University (4). The first section of Figure 2 shows the scores ob-

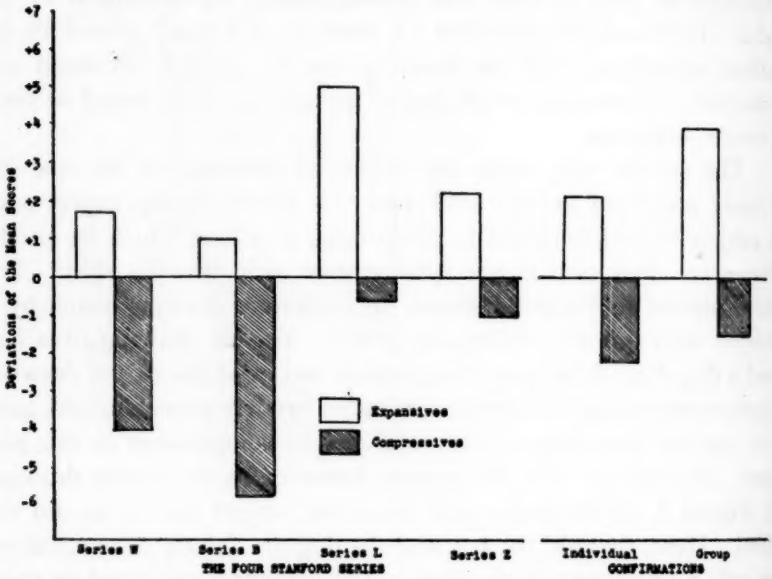


FIG. 2. Deviations of mean scores for the expansive and compressive groups of subjects in six clairvoyance experiments. The original expansion-compression study was based on the four Stanford series. Two later series carried out at Duke University with subjects tested individually and in a group situation, respectively, produced confirmatory results.

tained by the expansive and compressive groups of subjects in these four experiments. The bars on the graphs indicate the deviation above or below "chance" which the group of subjects obtained in the clairvoyance test. A deviation above "chance" indicates that the subject's drawings resembled the target pictures, while a deviation below "chance" indicates that the subject's drawings tended to avoid the subject matter shown in the target pictures. As may be seen, the expansive group of subjects in all experiments obtained a devia-

tion above the score expected by chance, while the compressive group in each gave a deviation below that expected. The difference between the scores obtained by the two groups of subjects (for the four series pooled) is statistically significant, with a probability of about 2 in 1,000 against such a difference occurring by chance alone. This initial discovery was based on the results of 96 subjects, 41 of whom were rated expansive and 55 of whom were compressive. Since these experiments, we have carried out two other clairvoyance series at Duke in which 95 subjects were tested (9). The results from these two series are shown in the second section of Figure 2. The two Duke series followed the same trend, the expansive subjects securing scores above "chance" and the compressive ones going consistently below "chance."

When the results from all six series are pooled, we find that the difference between the scores of the expansives and compressives is highly significant, with a probability of about 3 in one million. The total score of the expansive subjects, moreover, is significantly above the "chance" score, with a probability of 1 in 1,000. The total score of the compressive group of subjects, on the other hand, is significantly below the "chance" score, with a probability of 4 in 10,000. This latter score shows that the compressives tended to *avoid* the target pictures. Their response drawings did not bear a chance relationship to the target pictures as one might expect; on the contrary, the scores suggest that the compressive subjects were actually *rejecting* a true ESP impression and were drawing something dissimilar to the target picture.

It is interesting to note that the *total* results of all these series showed no evidence of ESP. The total deviation was slightly below chance expectation. Thus the expansion-compression criterion has revealed the presence of ESP where formerly it had been masked by the conflicting scoring trends of two different types of subjects.

So much for the clairvoyance results. The drawings next studied in relation to expansion-compression were those obtained in general ESP (GESP) tests in which an agent in a distant room looked at the target pictures. There were six such series available in the Laboratory, and I applied the expansion-compression criterion to these drawings in the same manner as for the clairvoyance draw-

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ings (5). The only difference in these series is that here the subject was primarily trying to make a drawing in response to the content of the agent's thought at the time. The data for the 239 subjects in these six series are presented in Figure 3, which shows the results

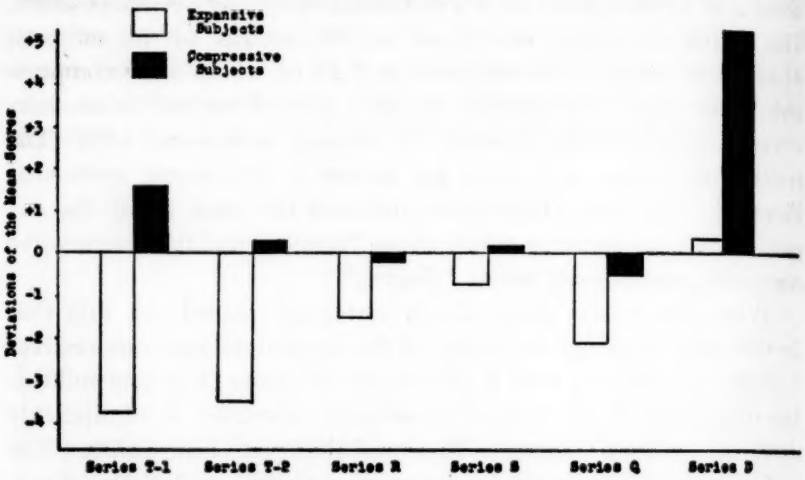


FIG. 3. Deviations of mean scores for the expansive and compressive groups of subjects in six general ESP series. (The names of the series are those given in the original report of this research.)

graphed in the same manner as for previous series. Here we see a striking reversal. Now it is the expansive subject who is scoring below "chance," while the compressive subject scores above "chance." In every single series the mean score of the compressive group of subjects is above that of the expansives. The difference between the mean scores of the expansive and compressive subjects for all six series is highly significant, with a probability of 3 in 1,000.

It is interesting to find that here also the *total* deviation obtained by all subjects on all of these series would not have been significantly different from chance had we not discovered this difference between the subjects. The scoring trends of the two groups of subjects canceled each other effectively so that the total deviation on all series was slightly below chance expectation.

The striking reversal of trends shown by the clairvoyance and GESP series offers a very perplexing problem which has not yet been solved. In trying to account for the observed differences we might speculate somewhat along the following lines. We might

suppose that the expansive person is one who is independent and rather self-sufficient. Therefore, he may particularly enjoy the challenge of a clairvoyance test where no agent is involved, and so he does well in such tests. When the expansive subject comes into the general ESP experiment, however, he may be somewhat bothered by the idea that someone else could intrude upon his privacy and that he might have to depend on someone else. Thus, he obtains a negative deviation in these tests.

From the type of drawings made by the compressives, we can see that these subjects are relatively timid and perhaps insecure. Thus, they might be somewhat frightened by a clairvoyance experiment in which they are entirely on their own. In the GESP test, on the other hand, where they have someone to lean on and to share the responsibility for the score obtained, they are able to do well. These are of course only speculations. We shall have to wait for future research to solve the mystery of this reversal of trend in the general ESP and clairvoyance series.

One question which immediately arises is: How easy is it to rate these drawings for expansion-compression? There are undoubtedly subjective elements involved in the judgments. Thus far, I have actually tried only a few people as judges, primarily for the reason that we soon discovered that Dr. Stuart judged the drawings in much the same way as I did. For instance, in the two clairvoyance series done at Duke, the expansion-compression ratings used were compiled by averaging the ratings made by both Dr. Stuart and myself. We made our judgments independently and found that we correlated very well. The correlation coefficients were +.67 and +.77 (for 32 and 63 cases, respectively) which show a high degree of agreement for material of this nature. Furthermore, we found that if we eliminated those cases on which we disagreed, an even more striking difference between the scores of the expansives and compressives was revealed.

In most of the other work which I shall mention, the expansion-compression ratings were based on the average of Dr. Stuart's and my judgments. Incidentally, I have tested four other judges and have found significant positive correlations between their ratings and mine; however, only one of the four had had any clinical ex-

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perience and his judgments are the only ones which correlate well enough with mine for practical purposes.

From a study of the expansion-compression ratings obtained by a single subject through several sessions, it becomes apparent that the rating is primarily indicative of the subject's mood of the moment. This aspect of the expansion-compression measure is very helpful in itself. It has generally been found in ESP experiments that a subject may score well on one day and poorly on the next. Now the expansion-compression criterion helps us to catch something of the subject's change in mood and also helps us to understand a little better the reasons for a subject's day-to-day fluctuations.

A study of expansion-compression ratings of subjects who have participated in more than one experimental session shows that subjects do change in their degree of expansion-compression and furthermore that their ESP scores reflect this change also. In clairvoyance tests, for instance, the scores are low when the subjects are compressive, and positive when they are expansive. Furthermore, it frequently happens that a subject is compressive on his first drawing or two; however, as he warms up to the situation and becomes adjusted to his task, he frequently begins to expand. His ESP scores also reflect this trend, starting out below chance expectation and rising to a positive score as the test proceeds.

Let us now review briefly a few other experiments in which the expansion-compression analysis has been applied. One of the graduate students in the Parapsychology Laboratory at Duke University carried out a series of drawings tests in connection with a special experiment of his (1). Each of the twelve subjects did two drawings with the clairvoyance procedure and two with the GESP procedure. Because of a special purpose for which this experiment was carried out, it was necessary to conduct the tests in a small, soundproof room in the basement of one of the buildings on the Duke campus. The room was dark and gloomy and in general the atmosphere was oppressive. It was interesting to find that all twelve subjects who participated in the experiment received compressive ratings. Although each subject participated in two experimental sessions, all of the drawings were consistently compressive. In the clairvoyance tests, these compressive subjects obtained a negative deviation,

while in the general ESP tests their score was significantly above "chance." The difference between the scores obtained on these two different types of ESP tests by these compressive subjects is also statistically significant, with a probability of 5 in 1,000.

These results suggest that the expansion-compression criterion may be very helpful in reflecting the effect of the experimental environment upon the subject. One sign of compression in drawings is that quality which gives a feeling of being shut in, of pressure, or of isolation. Thus it is not surprising to find that when subjects are tested in a small, dingy room they reveal their feelings by making compressive drawings.

From certain other data there is also the suggestion that the expansion-compression criterion may reflect something of the subject-experimenter relationship. In experiments conducted by some investigators, the proportion of expansive subjects to compressive subjects runs generally around 50-50. In the data of other experimenters, however, the majority of the drawings obtained will be compressive.

As a follow-up of the discovery that the expansion-compression criterion effectively separates the subjects into high- and low-scoring groups, we decided to conduct an experiment to see if the form quality of drawings also distinguishes between high- and low-scoring subjects in ESP card tests made during the same session as the drawings (7). Dr. Burke Smith carried out these experiments by giving clairvoyance tests to groups of his students in the classroom situation. The subjects were asked to make a drawing in response to an enclosed target picture, and then they were given two runs of clairvoyance card-calling tests. The same procedure was then repeated at the next class meeting two days later. The drawings made by the subjects were not scored for ESP, but were used only to provide an expansion-compression rating. These drawings were rated by both Dr. Stuart and myself. Then the ESP card scores of those subjects who were rated as expansive were compared with the card scores of subjects whose drawings were judged to be compressive.

Three different classes were tested with this procedure. Since each class was tested on two different days, there are the data from six experimental sessions to be considered. In every case, the ex-

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pansive group of subjects scored higher than the compressive subjects. The probability of the difference between the two groups of subjects for all the classes combined is 6 in 1,000. Thus in these series of tests we may say that the expansion-compression criterion successfully discriminated between high- and low-scoring subjects in an ESP test outside the drawing task itself.

In an interesting study following this one, Miss Elizabeth McMahan carried out an experiment in pure telepathy with some of the same subjects (**6**). For this purpose, I selected from the classroom clairvoyance series just discussed, 12 subjects who had obtained expansive ratings on their drawings and who had also obtained high scores on the clairvoyance card tests. Then I chose 12 other subjects from the same series who had obtained compressive ratings and who had made low scores on the clairvoyance card tests. The names of all 24 subjects were shuffled together and then were given to Miss McMahan so that she could ask them to participate in her pure telepathy experiment. The selection of these high-scoring and low-scoring subjects for her experiment was done by me so that she could have no knowledge of the subject's previous performance, since such knowledge could conceivably influence her treatment of the subjects and hence their scores. It must be kept in mind that the pure telepathy test, although rather difficult from the experimenter's point of view, seems to the subject to be practically the same as the general ESP test. In Miss McMahan's experiment the subjects recorded their impressions of the ESP symbol she was thinking of on each trial.

She discovered that the expansive subjects who had been chosen because they made high scores in the clairvoyance tests, scored well below chance expectation in the pure telepathy tests. On the other hand, the compressive subjects who had made negative scores in the clairvoyance tests obtained highly positive scores in the pure telepathy tests. The difference in the scores obtained by the two groups of subjects was statistically significant. These again were ESP tests in which the high-scorers and low-scorers were successfully separated on the basis of the form quality of drawings made outside the ESP test itself.

In recent months many other experiments involving card tests have been carried out in conjunction with drawings tests in the

hope that the expansion-compression ratings of the drawings would separate the high- and low-scorers on the card tests. Unfortunately the results have not always been as successful as those of these first series already discussed. On the whole, in the clairvoyance tests, the expansive subjects are consistently above "chance" and the compressive ones below "chance," but the differences in the scores obtained by the two groups are frequently not statistically significant.

However, a further analysis of some of these card-test experiments has revealed some interesting effects. All of the experiments consisted of group classroom tests conducted with undergraduate students at Duke University. Dr. Smith has carried out eight series of classroom tests, including the three series discussed earlier. In all of these the subjects recorded their calls for the ESP cards on record sheets containing space for two runs only. In all of these experiments also, the two runs served as a test unit; after the subjects had made their calls for the two runs, a checkup was made to allow them to see how well they were doing. The standard page, then, of two columns each, which served in all eight classroom experiments, provides a nice unit for the study of position effects. Following the procedure often used on PK data, I quartered these pages and compared the hits obtained in the first half of the first run with those obtained in the second half of the first run, and similarly with those in the first and second halves of the second run on the page. I then separated the pages for the expansive and compressive groups of subjects. The two groups of subjects showed quite different position effects. All of the clairvoyance classroom tests were analyzed first and their results are shown in the top half of Figure 4. Here it will be seen that the compressive subjects began very much below "chance" on the first run and hit their peak on the first half of the second run. It may be noted that the average number of hits obtained in this third quarter by the compressives is much higher than anything obtained by the expansive subjects (whose results are graphed to the right of those of the compressives). The first quarter of the page done by the compressives is very, very low. It is significantly different from the score expected by chance and is also significantly different from the average score obtained by the expansive subjects in their first quarter. The expansive subjects started out well in the first half of the first run,

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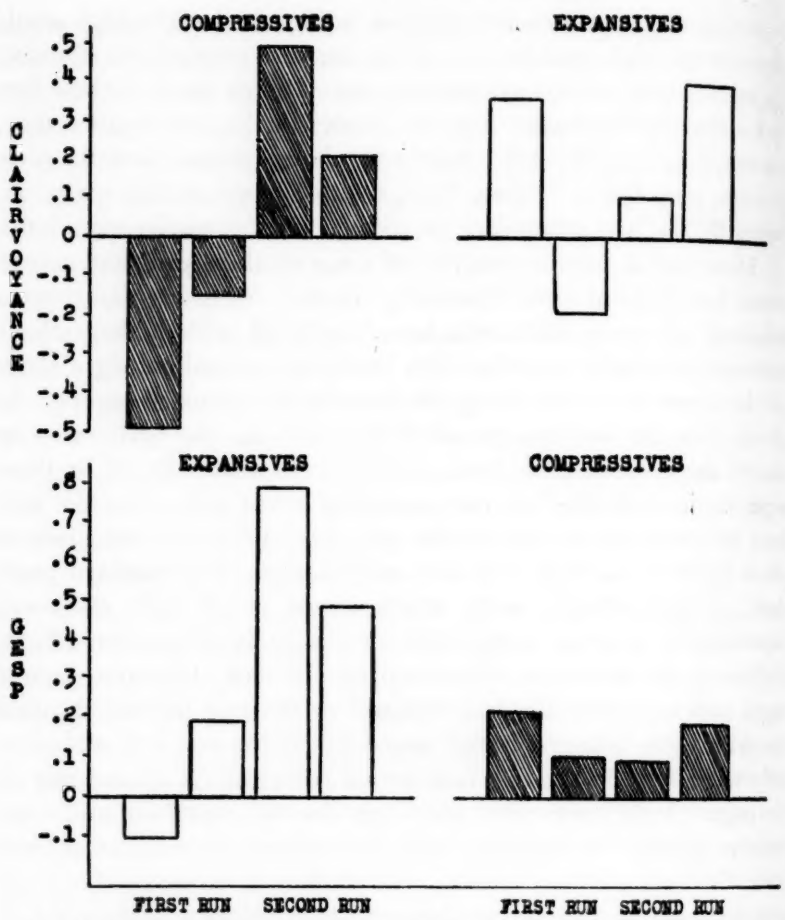



FIG. 4. Distributions of hits on the page for eight clairvoyance series (top half of graph) and for five general ESP series (lower half). The four bars in each section of the graph indicate the deviations of the average run scores for (reading from left to right) the first and second halves of the first run and the first and second halves of the second run on the record sheets.

and then declined to a below-"chance" level, and finally recovered their initial scoring only in the last half of the second run—that is, as they neared the end of the test unit.

When these results from the clairvoyance series were considered, I predicted that an examination of the general ESP test data would show an exact reversal of these patterns. This hypothesis was based on the reversal shown in the general ESP drawings experiments.

With five of the same classes that had been used in the clairvoyance series, Dr. Smith also conducted general ESP tests. When these data were analyzed for position effects for the expansive and compressive subjects, the results shown in the lower half of Figure 4 were obtained. Here it will be seen that the trend shown by the expansive subjects is quite similar to that shown by the compressive subjects in the clairvoyance tests. Similarly, although the deviations are not as extreme, the compressives show the same trend in the general ESP tests as was shown by the expansive subjects in the clairvoyance tests.

These findings suggest the usefulness of personality data in helping us to understand the various position effects which occur so frequently in parapsychological data. If the position effects in these various series had been pooled indiscriminately, we would have obtained an entirely different picture of the scoring trend, which would have been something of a statistical artifact caused by the canceling of trends shown by the two different groups of subjects.

There remains of course a great deal to be done in following up the various findings with the expansion-compression criterion. The most urgent need is to discover the reasons for the differences between general ESP and clairvoyance series. Finding the reasons for this perplexing reversal should afford us many valuable insights into the nature of ESP. The difference may hinge on the role of the agent in general ESP and pure telepathy experiments. It may be merely the "idea" of the agent in the subject's mind which causes the difference.

For another thing, we want to find out how the expansion-compression ratings correlate with other personality characteristics. So far in the few exploratory experiments that have been carried out along this line, no correlations have been found. Perhaps this is to be expected, since the expansion-compression criterion seems to be primarily a reflection of the subject's mood of the moment.

We also want to carry further our initial efforts to see whether the expansion-compression criterion in conjunction with one or more other personality measures, will prove even more discriminative of high- and low-scorers than does the expansion-compression criterion alone. So far we have found some evidence that this is so.

Another line of exploration which we are beginning to follow is

to explore the usefulness of the expansion-compression criterion in other types of parapsychological tests. For example, we are trying to find out if high- and low-scorers in precognition tests and in tests of psychokinesis can be separated on the basis of this criterion, and how the results compare with the clairvoyance and general ESP results.

In addition we are on the lookout for other measures of personality which will be as effective in separating high- and low-scorers as expansion-compression has been. I think we particularly need to find *other* measures which will be indicative of the subject's mood-of-the-moment, since practically every subject seems to have some inexplicable ups and downs in scoring. I would also like to see these "mood-catchers" used in conjunction with ratings on the more stable personality characteristics such as are provided by the Rorschach test.

Our ultimate hope is of course that we shall be able to discover the various personality correlates associated with high- and low-scoring and that we shall be able to predict in advance what sort of score the subject will make before he takes an ESP or PK test of any kind.

## References

1. Bevan, J. M. ESP tests in light and darkness. *J. Parapsychol.*, 1947, **11**, 76-89.
2. Elkisch, P. Children's drawings in a projective technique. *Psychol. Monogr.*, 1945, **58**, 1-31.
3. Humphrey, B. M. *Handbook of Tests in Parapsychology.* Durham: Parapsychology Laboratory, Duke University, 1948.
4. ———. Success in ESP as related to form of response drawings: I. Clairvoyance experiments. *J. Parapsychol.*, 1946, **10**, 78-106.
5. ———. Success in ESP as related to form of response drawings: II. GESP experiments. *J. Parapsychol.*, 1946, **10**, 181-96.
6. McMahan, E. A. An experiment in pure telepathy. *J. Parapsychol.*, 1946, **10**, 224-42.
7. Smith, B. M., and Humphrey, B. M. Some personality characteristics related to ESP performance. *J. Parapsychol.*, 1946, **10**, 269-89.

8. STUART, C. E. An ESP test with drawings. *J. Parapsychol.*, 1942, 6, 20-43.

9. STUART, C. E., HUMPHREY, B. M., SMITH, B. M., AND MCMAHAN, E. A. Personality measurements and ESP tests with cards and drawings. *J. Parapsychol.*, 1947, **11**, 118-46.

*Parapsychology Laboratory*
*Duke University*
*Durham, North Carolina*

## AFTERNOON SESSION

## ROUND TABLE DISCUSSION ON THE TOPIC: "THE NATURE OF PSI PROCESSES"

J. B. RHINE, *Chairman*

The speakers, who led the discussion[1] concerning the relation of psi processes to their various fields, were as follows:

Dr. R. A. McConnell, Department of Physics, University of Pittsburgh, Pittsburgh, Pa.

Dr. R. B. Roberts, physicist, Washington, D. C.

Dr. J. A. Greenwood, Principal Statistician, Bureau of Aeronautics, Navy Department, Washington, D. C.

Dr. C. B. Nash, Department of Biology, St. Joseph's College, Philadelphia, Pa.

Dr. Louisa E. Rhine, Parapsychology Laboratory, Duke University, Durham, N. C.

Dr. Jan Ehrenwald, psychiatrist, New York, N. Y.

Dr. Montague Ullman, psychiatrist, New York, N. Y.

* * *

DR. RHINE:

As everyone here probably knows, we have decided, at the suggestion of Drs. Thouless and Wiesner, to designate as "psi phenomena" all occurrences that come under the general heading of extrasensory perception (including telepathy, clairvoyance, and precognition) on the one hand, and psychokinesis (or the mental influence exerted over physical objects) on the other. Psi is loosely equivalent to the popular term, psychic.

[1] Since facilities for recording the discussion from the floor were not available, no attempt is made to reproduce that part of the round table here.

We have chosen the broad topic, "The Nature of Psi Processes," for this round table discussion because it is general enough to draw contributions from the many wide-ranging points of view that we shall find are represented here. We do not, of course, expect to have perfect agreement or final solution as an immediate result of this discussion. Such is not even our objective. Rather, we hope to get from these discussions some new problems to attack, some new ways of attacking old problems, and some encouragement and challenge to go on with what we are already doing.

When we inquire about the nature of a phenomenon, we have to specify our point of view. For example: What is the nature of the psi phenomenon with respect to the physical world? The biological world? And so on? For convenience we have broken down the general topic into a number of such subdivisions which will form the basis for the following papers.

It may be useful at the outset to review a few points on which everyone in parapsychology can agree concerning the nature of psi. First, psi is generally unconscious in its operation; it can be subjected to some degree of volitional control; it can be directed at a target; but ordinarily it is not *consciously* experienced. It is an extremely elusive and unstable ability and can, without the subject's awareness, function either in a positive or in a completely negative direction, and can hit or miss the target. It appears to be subject to normal motivation as far as it can be controlled, and it operates on a wide range of target objects. It is easily distracted, however, and is subject to many inhibitions. It appears not to behave in its space-time-mass relations as we expect a physical process to do, nor is it reliably known to be limited to a given species or group.

Our discussion may contribute something which can be added to this small beginning of well or only partly established findings concerning the nature of psi.

* * *

DR. MCCONNELL:

I would like to bring to the discussion three questions, to each of which I shall suggest an answer:

1. Can psychokinesis be explained by physics?

2. Does psychokinesis belong to the field of physics?
3. Does psychokinesis violate any basic principle of physics?

In answering these questions I shall interpret psychokinesis as meaning the direct control by the mind of falling objects such as dice or discs. This is the only kind of psychokinesis that has been demonstrated in the laboratory.

To the first question: "Can PK be explained by physics?" the answer is, no. Just as in the case of extrasensory perception, there is no known physical phenomenon which can be tied up with PK. There is no known physical principle by which it might be explained.

To the second question: "Does PK belong to the field of physics?" the answer is, yes. PK belongs to psychology *and* to physics. I think every physicist would agree that to control the fall of dice is to operate in the domain of physics.

The third question: "Does PK violate any basic principle of physics?" Again it is possible to give a definite answer. That answer is, no; PK does not violate any basic principle of physics.

Many people will not be satisfied with that answer. Three counter-arguments are commonly heard. First, we know so much about physics that there is no room left for PK, and there are no gaps between the known facts big enough to accommodate it. Second, PK experiments are undependable and non-repeatable. Physics, on the other hand, is a well-behaved subject. How can you reconcile the two? Third, PK is contrary to common sense.

Let us look at these three arguments one at a time. First the statement that there is no room in physics for PK. I doubt that any physicist would seriously propose this argument. There is nothing in history which would lead us to believe that we can know when science is complete. Quite the contrary: we know from past experience that there must be many second- and third-order effects still to be discovered. Since PK, as it occurs in everyday life, is only a marginal effect, it should be rather easy to accept it as a high order aberration which is observable only under peculiar conditions.

The second argument against PK springs from the fact that, as yet, no one can guarantee to repeat an experiment successfully. Physicists are likely to feel that this constitutes a serious criticism of PK. I think the sensible position to take is that non-repeatability is grounds for suspicion but not for rejection. We must remember

that repeatability is a relative thing. No experiment is truly repeatable. We physicists are very lucky; in most of our work we can get the same answer by controlling only a few variables. But PK is only partly physics; it is also partly psychology. That makes the problem more difficult. Strictly speaking, no experiment in any science is exactly repeatable. One of the problems in any science is to discover the important variables and how to control them. This problem is still largely unsolved in parapsychology.

The third charge commonly made against PK is that it contradicts common sense. I am inclined to agree: PK *is* contrary to common sense. At the end of the nineteenth century I don't think PK could have been accepted by physicists. At that time physics was largely regarded as common sense expressed in decimals. Since then physicists have been disillusioned. In 1905, for example, we were given Einstein's special theory of relativity. Relativity was nonsense then, and it still is. But it is a very real kind of nonsense, a kind of nonsense that works. Modern physics doesn't care whether an idea violates intuitive reality. Modern physics asks only one question: "Has it been observed?"

PK belongs to physics and is, therefore, in part a responsibility of physicists. We cannot explain it, but by our own principles we must accept it unless we can find some flaw in the evidence.

* * *

Dr. Roberts' remarks were substantially as follows:

The particular challenge of psi phenomena arises from the fact that they apparently lie outside our general framework of knowledge of the physical world. Of course there are many other problems in science which remain to be solved, but there is usually little doubt that a solution will eventually be found within the present framework. With psi, on the other hand, it is apparent that the old outlines must be radically expanded. There is no conflict between the phenomena observed and the laws of physics since these phenomena lie outside of the domain of physics.

In the older fields of perception, physics has contributed by its study of optics, acoustics, etc. In the newer field of ESP the physicist would like to investigate the laws of transmission, the space and time dependence, leaving the study of the human factors to the psychologist. At present, however, human subjects are the only avail-

able instruments and the human peculiarities seem to have a much greater influence than the physical factors. The situation is comparable to measuring the characteristics of golf balls by observing the drives of Sunday golfers. The only hope for better instruments would appear to be the use of animals as subjects. These might be free of some of the complexities of human subjects and would bring a major advance in psi technique.

In spite of the difficulties arising from the techniques, certain aspects of the situation are clear. Whatever the ultimate interpretation of psi phenomena may be, it certainly will not be in terms of physics as we know it today. Any ordinary transfer of information involves the transfer of energy. This may be carried by material particles or by radiation. Enough is known of their properties to rule out either possibility conclusively. There is no room in the spectrum for any new radiation of the required characteristics.

In the future even though more is known about psi phenomena there probably will be no "explanation" of them. The physical sciences have boiled down and condensed the mysteries of nature so thoroughly that many people are apt to forget that they still exist. For example, we can explain the motions of planets, falling bodies, etc. in terms of the law of gravitation; but the law of gravitation is completely unexplained. It is familiar, we can describe its action, we can use it to predict certain events, but we do not "explain" it. So also with psi. In the future there may be laws and predictions, but not explanations.

* * *

DR. GREENWOOD:

Dr. Greville, in his paper to the symposium during the forenoon, discussed current uses and proposed applications of mathematics and statistics in parapsychological research. To avoid covering the same material again, I will limit my discussion to some evaluative techniques that might with profit be added to those he mentioned.

In the short time at my disposal I wish to call attention to three statistical techniques that have thus far been used relatively infrequently in the study of psi processes. Without exception these are well-tested and familiar methods to statisticians. In a sense they have not been necessary in parapsychology up to this point. Now,

however, as the research workers are delving more deeply into the interrelations among the variables possibly affecting psi performance, not to mention the study of such intangibles as mediumistic utterances, some of these techniques may be found more useful.

One of these statistical techniques is the use of the contingency table. A situation in which this might be useful, for example, would be that of comparing results on a probability basis from token object tests by the same subject when some objects belonged to living persons and others to deceased individuals. We can easily design the test in a way that puts the analysis on a sound basis for inference. In general this method is useful when variables are assignable into categories. Thus the class of objects selected can be arbitrarily taken from two categories, living persons and deceased persons.

The second statistical method I wish to mention is the multiple regression technique. This is particularly useful when the separate variables and the results are measurable. For example, one might apply this method in studying a possible dependence of psi results upon temperature and humidity. When the situation is such that the variables cannot be assigned (that is, given arbitrary values), this method enables us to find the contribution of each of the independent variables to the total effect observed.

Finally, the analysis of variance method has its special advantages. This is particularly appropriate when the independent variables can be assigned values at will in the experiment. With an appropriate experimental design one can thus estimate the effects of each variable on the results as well as the interactions of these variables while covering a large range of experimental conditions. The way is open in some cases for an easy and efficient procedure to estimate the linear and higher order regressions, such as the steady decline in success within the run or the U-curve effect.

These methods generally weaken the hypotheses required to make an inference, as compared to earlier methods used, as they make no assumption regarding the a priori probability of success on the single trial. This has the effect of logically strengthening the conclusions. The *t*-statistic, which is used in much of the current psi analysis, has the same logical advantages as the methods discussed briefly herein.

* * *

DR. NASH:

Biology merges with psychology in the science of comparative psychology, and with medicine in the science of physiology. Without attempting to decide whether the particular psi phenomenon falls within the field of biology proper, or in the field of comparative psychology, or the fields of physiology and medicine, it is possible to divide psi phenomena from a biological viewpoint into the following three groups: (1) psi phenomena of the individual organism, (2) intra-specific psi phenomena, and (3) inter-specific psi phenomena. PK and ESP will be considered separately under each of these three divisions.

*First, psi phenomena of the individual organism.* As regards PK in the individual organism, it would be desirable to determine whether stigmata (marks miraculously produced on the bodies of certain persons, corresponding to the wounds that Christ received) are produced through the agency of PK. Similarly, diseases and disorders may sometimes be due to PK, and, on the other hand, miraculous cures and some of the work of the Christian Scientists may be PK phenomena.

PK in the animal kingdom may be an adjunctive cause of evolution. Striving of the organism to adjust to its environment may occasionally produce inheritable changes which provide raw material for the process of natural selection.

Finally, a study of PK in the individual organism from a biological viewpoint should include a study of the untested claims of the physical phenomena of mediumship.

As regards ESP in the individual organism, it should be determined whether an animal can by ESP select a desired object more frequently than a repulsive object, both objects being similarly disguised or hidden. It should be determined how far down the evolutionary scale psi phenomena exist in animals.

A problem already recognized by biologists is the riddle of migration of animals, particularly birds. How they find their way over great distances in darkness has not as yet received a scientifically accepted explanation and may be due to clairvoyance.

*Second, intra-specific psi phenomena.* As regards PK between individuals of the same species, there is the problem of black magic

in man; also the possibility of pre- and post-natal influence of the mother on the child. Even the sex of the child may occasionally be determined by PK through control of the particular sperm that fertilizes the egg (the sex of the child being determined by whether the egg is fertilized by a sperm carrying the factor for maleness or by a sperm carrying the factor for femaleness).

As regards ESP between individuals of the same species, the possibility of the occurrence of telepathy between animals is of biological interest. Mutual location of oppositely sexed individuals, of parents and young, and of members of species which aggregate may be facilitated in animals by ESP.

*Third, inter-specific psi phenomena.* As regards PK between individuals of different species, a biological problem of the first importance is to determine whether man can influence the physiological activities of animals and plants by PK.

As regards ESP between individuals of different species, it would be desirable to determine whether telepathy exists between master and dog, and between dog and master. Also of biological import is the possibility that ESP may occasionally facilitate for predatory animals the location of their prey, and facilitate for prey the evasion of their predators.

If it is established that the physiology of the body can sometimes be affected by PK, or that man can at times affect lower organisms through PK, or that ESP at times occurs in lower organisms, psi phenomena will have an immediate and basic place of great importance in the biological sciences.

* * *

DR. LOUISA E. RHINE:

Reports of spontaneous psychic occurrences do not in themselves constitute data upon which to base conclusions. Nevertheless, a survey of such reports can give many helpful suggestions to the experimentalist, as, for example, in studying the nature of psi and its relation to consciousness. In experimental work, ESP seems to operate on the unconscious level so that the percipient does not know which of his responses is right. Of course, it is highly desirable to find some way of raising the process to the conscious level, for only so is there much hope of learning to control it. For if it is a

necessarily unconscious process, there wouldn't be much chance of doing that.

A survey of spontaneous case reports, however, gives much encouragement on this point. For in these reports we find what seems to be a much greater range in degree of consciousness of the ESP impression than occurs in experimental work. While we do find much evidence that the impression has difficulty in crossing the threshold into consciousness, just as shown in the laboratory, we have in addition the testimony of many cases which seem to fall at the two extremes.

First, showing least consciousness and most fragmentation of the ESP impression, there is the type of case which we call blocking, in which nothing specific whatever gets over the threshold. The percipient experiences a vague unrest, or depression, or other appropriate emotional state or impulse to act, but does not even know to whom or to what his experience applies. Then, linking this type up with the more intermediate degrees of fragmentation, we find cases where the only conscious knowledge which comes through is the identity of the person involved. Following these, we have practically all stages up to the opposite extreme, which consists of the cases which show what we call conviction. In these, knowledge seems to cross the threshold into consciousness practically in entirety, and sometimes with such conviction of its validity to the percipient as to warrant a radical course of action based upon it.

And so from the survey of cases, we feel encouraged to think that the ESP process is not necessarily an unconscious one, and that, therefore, there is a possibility of learning what its barriers to consciousness are, and so, how to remove them.

There is something also to be gained from surveying the manner in which the emergence into consciousness seems to be accomplished. Such a survey soon shows that processes already familiar to general psychology are common.

For example, we find cases where the association of some object or situation present in the environment of the percipient seems to make the emergence of the ESP impression easier, just as it sometimes does in the familiar process of recall in memory.

We find also some of the devices familiar in ordinary dreams. An outstanding one of these is dramatization. In this, the incoming

knowledge is acted out as on a stage. This may occur in the waking state as well, but in that state it seems to be more common to have the knowledge come to consciousness via the process we call symbolization. An omen or sign of some sort is experienced which conveys, by inference, the truth that is having difficulty of entrance. This sort of symbolization, of course, does occur in dreams also, but thus far in our study it seems to occur more frequently in waking experiences, and the dramatization more frequently in dreams. It will be interesting to see how far this difference holds as the study progresses.

Among other suggestions we are finding is that sometimes a definite disguising of the incoming impression seems to occur, as if to shield the percipient from shock. This, too, of course, is familiar in ordinary dreams, although in the spontaneous psychic occurrence it seems to occur either in waking experiences or in dreams.

All these types of processes, and others, and combinations of them are well represented in reports of spontaneous occurrences which show them as a part of the normal cognitive system, so that even in our initial studies, it looks more and more as if psi is lawfully interrelated with processes more familiar to orthodox psychology.

These are a few samples of the first fruits of our study of spontaneous cases—a study, we think, which may help us still further in an understanding of the nature of psi.

* * *

DR. EHRENWALD:

I suppose you don't expect the psychiatrist to contribute a significant statement on the nature of psi phenomena to your discussion. On trying to do so at the present stage he would only run the risk of changing places with some of his patients. But he can do three things. He can study the psychodynamics of so-called spontaneous phenomena; that is, he can apply to them the established principles of the psychoanalytic approach. You will hear more about that from Dr. Ullman in the course of our discussion. Secondly, he can try to arrive at certain generalizations about the *modus operandi* of the phenomena; thirdly, he can seek to reconcile his findings with the current system of psychiatric thought and to formulate a working hypothesis for further research.

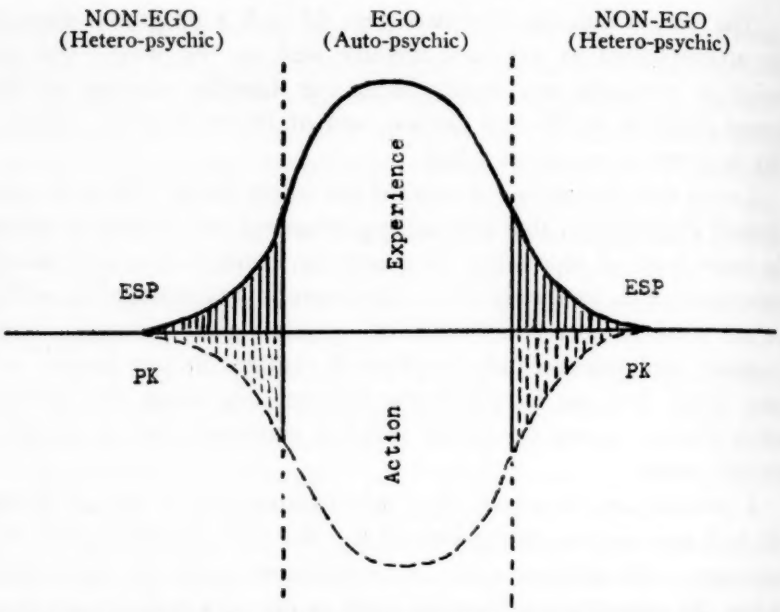
The fact is that the first two steps of such a program—though not accomplished as yet—are already well on the way. But on trying to reconcile our results with our familiar concept of the human mind in health and disease, and of the mind-body relationship in general, we are at a loss.

Let us take telepathy. I pointed out in my book, *Telepathy and Medical Psychology,* that telepathic phenomena are subject to much the same laws of repression, displacement, condensation, secondary elaboration, etc., as the dream or the neurotic symptom. But while we are used to correlate "ordinary" psychological processes, both conscious and unconscious, to physical changes in our brains, we must admit that we have not the faintest idea about the way in which what is going on in my mind is communicated to another person's mind.

I pointed out, however, that this striking gap is not at all so odd as it may appear on the face of it. We have to realize that the same gap—although not quite so conspicuous—exists in our notion of how the transmission from an event in my *own* brain to my consciousness is effected. As Professor Joad put it in a review of my book: "We forget the mystery of the latter (of what I termed *auto-psychic* experience) because it is common; we are astonished by the oddness of the former (of what I called *hetero-psychic* experience) because it is rare."

It is true that auto-psychic experience is but a special instance of the mind-body relationship. We are used to think of it in terms of a person's strictly "personal" mental processes, localized in his individual brain. We are, of course, fully ignorant of the nature of this assumed cerebral localization, but we are usually satisfied that this is the only legitimate way in which mental processes are supposed to occur. However, the available evidence for telepathic phenomena indicates that, on certain occasions, this exclusive functional relationship is supplemented by a "localization" involving a relationship between my own consciousness and another person's brain.

Such cases are admittedly rare and can at best be demonstrated by statistical mass experiments of the Duke type. This state of affairs is illustrated by the diagram below, representing, as it were, the frequency distribution of auto-psychic, as compared with hetero-psychic, experiences and actions.




Diagrammatic picture of the ego, represented as an "open system," merging into the non-ego. The shaded part is the area overlapping with other egos, termed the *hetero-psychic* as against the *auto-psychic* sphere. The probability, statistically speaking, of auto-psychic experiences (and actions) is infinitely greater than the probability of hetero-psychic experiences and actions, that is, of ESP and PK.

The central zone of the graph is occupied by the familiar category of mental content with its clear-cut and exclusive relationship to my brain. This is flanked by a decreasing number of mental events in the marginal zones which I may or may not share with other persons in a telepathic way. They are represented by the gently sloping ends of the curve.

Applying to the diagram the knowledge gained from our psychodynamic approach, we can say: the higher the emotional significance to us of a mental event pertaining to another person's brain, the greater, statistically speaking, the probability that we may get aware of it in a telepathic way. Conversely, the less my need to relate myself to him on an emotional plane, the more improbable is such an event.

It will be noted that, with some modification, this formulation can be applied to events both in another person's brain and to events in "mindless" bodies, i.e., in physical objects in the outer world to

which we are able to relate ourselves. In short, it covers both telepathy and clairvoyance.

But this is only one side of the picture. We can plot an identical frequency curve with regard to the motor side of our mental functioning. The central area of such a curve would be occupied by such familiar events as my will power, effecting a contraction of my right index finger. Only rarely am I able to control my own breathing or heartbeat in a similar way. I am still less likely to be able to contract the muscles of my paralyzed arm, although we know from clinical experience that a hemiplegic may be able to do so when he is animated by a powerful emotion. Statistically more improbable still is our ability to effect certain movements of dice thrown in PK experiments. But the fact is that we are able to do so, even though on very rare occasions only.

We arrive in this way at a new conception of the so-called mind-body relationship, involving two or more bodies and minds related to each other within the context of an interpersonal emotional field. At the same time we have to discard the classical notion of a *closed and strictly isolated system of personality,* conceived in terms of materialistic monism, psycho-physical parallelism, epiphenomenalism, or what not. This concept has to be replaced by an *open system of personality* involving what might be called a mind-body gradient or psycho-physical gradient, as represented on our diagram.

To put it in other words, we have to supplement the familiar notion of an exclusively mono-cerebral localization of consciousness with the concept of a *potential pluri-cerebral representation of its marginal, preconscious, or unconscious sphere*—leaving the question of the nature of consciousness out of our frame of reference. This proviso is necessary because we have to admit that we know next to nothing of how the postulated pluri-cerebral processes are registered in our minds. But I may recall that we know just as little about the way in which we get aware of the exclusively mono-cerebral processes in our brains.

* * *

DR. ULLMAN:

In this brief presentation I wish to call attention to four observations which, in my opinion, have a bearing on the nature of the psi process as it is seen to operate in the psychoanalytic setting. Before

doing this, however, I wish to state my own bias. Thus far in my progress toward understanding I remain very distrustful of a dualistic approach to the study of natural phenomena. It is also my feeling that where a dualism seems to emerge from the facts, the chances are that it was lurking about somewhere before the facts put in their appearance.

What are these observations? How do they relate to the experimental findings? What new clues, if any, do they provide? The observations relating to psi and personality which I have in mind are as follows: (1) the indications from the work of Dr. Schmeidler and others (relating psi ability to attitudes) that emotionally free individuals seem to do better than the more inhibited types; (2) the fact that in my own experience very ill individuals teetering on the brink but not yet over on the psychotic side often indicate remarkable psi ability in the course of analysis; (3) the fact that once psychosis, or the complete absence of effective relationships with other people, sets in, the indications at present are that, at least in the experimental situation, psi functioning is not remarkable, nor is it in the clinical situation in my own experience; and (4) the consistent clinical fact that psychotics in their fantasies make elaborate pretensions at psi ability, sometimes quite openly, sometimes in more disguised form.

Before correlating these observations I want briefly to outline the meaning I give the terms "health" and "ill health" in psychiatry. We speak of people as balanced or unbalanced and this is a very apt description, embodying as it does the concept that as a result of opposing forces, strivings, and impulses either an equilibrium is achieved or it is not. The individual from birth on attempts to move forward without upsetting this equilibrium. He seeks to express, develop, and perpetuate his existence as a human being. To do this he must engage in constructive activity with other human beings. Constructive activity may be defined as that type of activity which enables the individual to fulfil his own needs at the same time that he is answering the need or needs of his partner, enabling both individuals to progress and engage in further constructive activity. The paradoxical outcome is that the greater the interdependence of one human being upon another, the closer the individual gets to the fulfillment of the potential for uniqueness and individuality.

The exact nature of this interrelatedness, this interdependence, is

as yet unknown. We do know that human feelings function in the service of this interrelatedness; perhaps psi ability does too. This speculative concept would favor the view of psi as an adaptive function making for awareness of threats to the human environment from the outside (clairvoyance) and awareness of threats from the human environment itself (telepathy). As with all other biologic abilities, once established it enters into the service of human needs.

Let us consider the first observation—that emotionally free individuals seem to do better in the experimental situation. Here the situation, either spontaneously or by design, creates certain needs, *e.g.,* to compete, to engage in a stimulating activity, to gain a reward, etc. The individual most free to fulfil his needs in general is most free to engage fully and successfully in situations which can only be hurdled by psi ability. It may be that psi in the service of constructive needs is most dramatically shown in the many instances of extrasensory awareness of death or injury to loved ones at a distance. Here the psi function is not only appropriate but necessary, and it may be that its necessity under these circumstances, as contrasted with the lesser necessities of the experimental situation, accounts for the clarity and definiteness in the one case and the more elusive and less impressive occurrences in the other case.

Before taking up the second observation let us return to our premise that by health we mean the ability to engage in constructive activity with other people. Thus ill health is the impairment of this ability, and the extent to which it is impaired is the extent to which destructive relationships are instituted. Neurotic trends are the representation of these destructive relationships. Let us consider what happens to another biologic function, namely, the sexual needs, as one goes down the scale of health. In the healthy individual the sexual drive is integrated in the service of a generally constructive relationship with an individual of the opposite sex. To the extent that ill health prevails the drive is pressed into the service of exploitive and other destructive trends. The greater the measure of ill health, the more the drive is seen to operate in a selfish or inhuman kind of way until all human ties are severed, and sexual activity loses all of its human aspects and degenerates into a socially useless kind of activity. Unless checked, this results in a gradual impairment of the ability to obtain satisfaction from human activity

and reliance is placed solely on the pleasure obtainable from a narcissistically oriented biologic activity.

The second observation would depict a somewhat analogous situation in relation to the psi function. In my own experience, overt psi functioning is met with most frequently in the most seriously ill patients where it at one and the same time serves the exploiting needs of the personality and also represents a last desperate foothold of relatedness. In the light of these formulations the third and fourth observations can be readily understood: where the individual has withdrawn from all relatedness fantasy supervenes and he is relieved of the necessity of expressing his needs in any kind of a real way. There are delusions about telepathic ability without the accompanying need to express it in a real fashion.

Investigation into psi gives promise of remarkable new insights into personality. Our experience thus far, contrary to indicating that a dualism exists, points out the impossibility of dealing with psi out of the context of human needs and the material origin and bases of these needs. When psi functioning does take place it can only be understood in terms of the human needs operating in a particular interpersonal situation at a particular time.

## THE PLACE OF PARAPSYCHOLOGY AMONG THE SCIENCES

By Gardner Murphy

### I

In accordance with the modern temper, facts have to be grouped together and formed into systems to make any sense to us. One science after another has taken shape as this process of observing and grouping and interrelating has gone on. Usually the different groupings hang together in still larger groups. Physics, chemistry, geology, astronomy hang together in the larger group of the physical sciences. Anatomy, physiology, embryology, etc. hang together in the larger group of the biological sciences. There is a strong impulse to pull all sciences together. In the large area represented today, for example, by the various subdivisions of experimental biology, there goes on an effort to cross-articulate the biological and

the physical sciences, and to see life processes in intimate relation with the processes of the physical world. The social sciences, too, have within recent decades tended not only to be grouped together, but to be brought into closer and closer relation with the biological, and hence with the physical, sciences.

Psychology arose in an atmosphere demanding this type of unity both of methods and of concepts. Experimental psychology on the one hand, evolutionary psychology on the other, and finally clinical psychology, as represented especially by psychoanalysis, have all striven to see the living system of the individual in terms of a general dynamic which goes to the heart of cosmic structure. For a unified view of the world was at last conceived by nineteenth-century science. It was a world of interacting particles located in time and space.

Nevertheless, there were and there remain important "odds and ends" which do not fit into the system. Just as there are many human beings that cannot fit into the existing social patterns, so there is a wide penumbra, or marginal area, of facts which do not fit into any existing category. It is not as a rule possible simply to sweep these scattered facts together into a new category—a sort of rag bag of odds and ends—for our whole modern temper demands unity, and the result of our having no well organized framework to receive such observations is that they simply do not get included at all. It does not follow that a scientific period will always look for and make the most of all such facts as can be discovered; woe be to those facts which find the pigeon-holes arranged, but none ready for them!

It was in this very predicament that the great pioneers of parapsychology found themselves when, a little past the middle of the last century, they kept encountering phenomena which did not suggest that the clue really lay in particles interacting in time and space. Telepathy, clairvoyance, prophetic dreams, hauntings, apparitions, were attested by scores upon scores of serious people (3). Witnesses frequently corroborated one another's observations as to paranormal occurrences, and many conducted their own experiments in telepathy at long distance. In general the learned world was not even interested to read, much less to discuss, the nature of such findings. And while telepathy might conceivably be a case of physi-

cal radiations, most of the phenomena did not fit, and have not fitted, into the accepted world-scheme. Clairvoyance, for example, does not appear to depend on radiations, nor can we conceive a dynamic for precognition. Nevertheless, there was some feeling that such facts demanded investigation, and societies for psychical research were founded to investigate and to systematize.

Investigate they certainly did; systematize they certainly did not. Year after year more and more evidence for telepathy was accumulated. Much paranormal material began to come through trance mediumship, such as that of Mrs. Piper (4) and of Mrs. Thompson (14), which suggested that, at the least count, information was coming from the sitter and from distant living persons, and perhaps even from the deceased.

Occasionally a heroic effort to develop systematic theories was made (6). But in general there was no way of testing these systematic theories, with the result that constantly new theories were offered, while more and more scattered observations accumulated; in consequence there was an ever-widening area that had to be mastered by the psychical researcher but with less and less chance of a unified system.

To add to these difficulties, one must stress that in general the observations were difficult to repeat, even by those who had first reported them. The long-distance experiments in telepathy in the early years, for example, were for the most part published as something bizarre or intriguing; and in general exact repetition under the same conditions at a later date was not attempted. Almost never was a systematic set of observations repeated independently by another investigator in another laboratory. As a matter of fact, even since the reconstruction of method in the field by Rhine (8) in 1934, relatively little has been done to repeat systematically even the best of these single experiments initiated by Rhine or his co-workers. Nothing would be gained by making too sweeping a generalization, and indeed a few experimental effects have been partially confirmed; say, for example, the "decline effects" which sometimes appear in both ESP (8) and PK studies (10), and the salience effects (9) which may appear at the beginnings and endings of ESP runs. But the fact remains that many have failed to find decline effects or salience effects even where the conditions would lead us to expect

them; and to say under what conditions a result will appear is one of the first requirements if we are to speak of true repetition.

The two things, then, that make the greatest trouble for parapsychology are, first, the fact that it deals with materials which are taboo from the point of view of the present world frame-of-reference; second, that its facts are only to a very limited degree repeatable independently in different laboratories. Either of these alone would be serious. The two together have kept the infant science long a-borning. It is proving to be a very difficult delivery.

II

Yet we have observations to deal with, the tough stuff of which science is ultimately composed. What, then, are the observations upon which parapsychology depends? They are broadly of two classes. (1) Instances in which the living organism appears to make contact with events outside of itself by means other than those now understood in terms of sensory physiology. (2) Instances in which the living organism apparently influences its environment without utilizing the equipment ordinarily involved in getting action through the physical world. The research activities which signalized psychical research in the eighties of the last century comprised both these types of phenomena. Just as is the case in many other sciences, the primary data were *spontaneous;* that is, they arose in unplanned situations. For that reason the early societies for psychical research concerned themselves with the discovery of instances of spontaneous telepathy, apparitions, hauntings, and the like, or instances of the movement of objects, as in physical mediumship or poltergeist phenomena; they set up committees and standards for the collection of observations; they grouped, interpreted, and published. First-class men participated in all these endeavors.

The *experimental* aspect of the work was very limited indeed. While experiments in telepathy were carried out in England and on the Continent, and a little later in the United States, no large-scale, well planned systematic experimental investigations of paranormal cognition with suitable statistical controls were undertaken until the time of Rhine's 1934 report. The case was the same as regards the physical phenomena. Sir William Crookes (2) had observed such phenomena even in the 1870's; they were again and

again observed in connection with physical mediumship. Actually it was not until the Duke University program that systematic, orderly, quantitative experiments on such physical effects were launched.

III

We are faced here with a very fundamental issue of method. To attract the attention of science one has to emphasize *experimental method,* while Nature is often shy or even negative to our determination to break up her data in this way. Despite this differentiation of an experimental field within a larger field of spontaneous events, it must always be borne in mind that the separation between what is spontaneous and what is experimental is one of degree, and one of design; that frequently Nature gives us in her eclipses, her meteoric displays, her Grand Canyons, phenomena which are of the greatest possible importance in systematizing data. And the role of experimentation may be either large or small without influencing particularly the structural form of a science. The geologist may take some of his materials into the laboratory and experiment upon them. The astronomer may plan in a true experimental spirit what observations to make at the critical moment in an eclipse; but this is not experimentation. Not experimentation as such, but orderly and systematic attack, with independent confirmation and verification, makes science.

But however true it may be that spontaneous *phenomena* and experimental *phenomena* are ultimately the same events in Nature, the attention of scientific men in such an area could not be caught by spontaneous cases. Consequently there is no special reason to complain or to be surprised that the attention of the scientific world during the early period was slow in coming. When scientific attention finally did come, largely in response to the huge research program at Duke, the criticisms of method went into all sorts of detail, for by invalidating the method one could bypass the whole subject; and much debating back and forth ensued (**11**). (In Britain, apparently, hostility has been a little less intense than elsewhere, and a little less effectively organized, partly because of the list of celebrities associated in that country with psychical research, and partly perhaps because of a certain tendency to diffuseness, isolation, and independence which has in general characterized British science, in

contrast with the much more orderly united-front type of approach characterizing Germany and the United States.) A dent was nevertheless made on the basis of an organized, long-range experimental attack. The issue was defined by Rhine in the definition of parapsychology as an *experimental* field of research within the broader domain of psychical research; and in spite of difficulties it has been this systematic laboratory investigation which has now spread to other institutions.

If, in spite of the difficulties and the basic fact that most experiments still remain non-repeatable, one insists that parapsychology is in the slow process of gaining admission to the sciences, the reasons for such an assertion are to be found, first, in the systematization and improvement of experimental methods, so that they are in some measure being copied and extended from one institution to another; and second, in the fact that for a good many years primary attention has been given, not to the simple repetition of tests of ESP or PK simply to see if they can once more be shown to exist, but to the *dynamics* which, when fully understood, should ultimately permit their control. It is in this spirit that the personality studies of Stuart (**13**), Humphrey (**5**), Schmeidler (**12**), and others have been developed, and it is in this area that some of the most interesting promises of repeatable experiments have appeared. Humphrey, for example, has obtained clear-cut quantitative evidence that her expansion-compression factors in personality operate differently in the case of clairvoyance and in the case of general extrasensory perception; while Schmeidler has repeatedly shown various types of personality factors in the Rorschach test, which appear to have predictive value for ESP scores. It may well prove to be a fact that repetition of the *external* conditions can give us no repeatable paranormal phenomena, while repetition of the subtle interpersonal relationships, the release of dynamic factors on both sides so that the field relationships are relatively constant, can give repeatable paranormal effects.

## IV

Yet no significant extension of method is possible except on the basis of an extension of theory. My own view would be that not too much but too little attention has been given to systematic theory-building, from which specific research hypotheses may be derived

and from which ultimately experimental materials may be assembled. Two illustrations will perhaps be sufficient: 1. Margaret Reeves' (7) ingenious elaboration of Kurt Lewin's topological psychology to afford a comparison of the differences between experimental and spontaneous telepathy emphasized deep-level dynamics in the personality as compared with more superficial dynamics, so as to show why the experimental procedure draws from the individual at a level which is ordinarily quite different from the one which functions in spontaneous cases. 2. Whately Carington's bold hypothesis (1) regarding the association theory, which guided his series of experiments, made it possible to show that the more frequently certain stimuli had been associated together in the minds of the agent, the more likely they were to be associated in the minds of percipients. These hypotheses seem to me to represent real progress toward a systematic conception of the paranormal.

## V

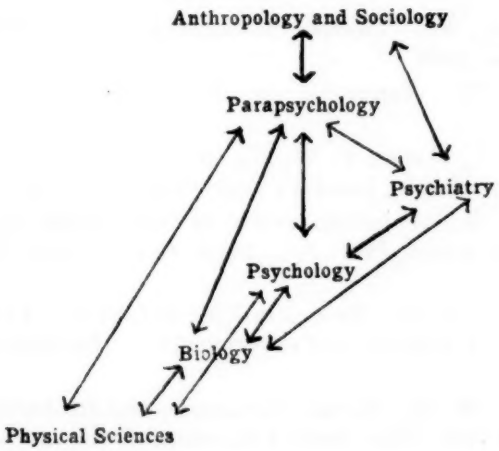
Just as important, however, as success in theory building is the problem of success in the recruitment of personnel. For a good many years the chief recruitment has been in terms of scientists who have given some time to parapsychology "on the side"; men and women who have already achieved training in some other science have turned some of their attention to parapsychology. This is true, for example, of Soal, Tyrrell, Carington and Thouless in Great Britain, true of McConnell, Greville, George, Taves, Martin and Stribic in the United States. There has been a good deal of recruitment of young psychologists in recent years under Rhine's influence. The great problem remains, ot course, the question of financing their life work; and this kind of task, with its huge difficulties, cannot be done on a shoestring, so that problems of personnel are therefore largely problems of finance. It has proved possible through a system of fellowships and other staff appointments to develop a small research personnel in parapsychology at Duke University. But to place these people in positions where they can permanently carry on a research program in parapsychology has been difficult indeed. We are caught as usual here in a circular situation. We could get huge foundation support for this type of investigation if we could produce repeatable experiments which could

be done in centers where the foundations are interested in carrying work forward. Until, however, the ice is broken, it proves extremely difficult to get any sort of foundation interest. We continue, therefore, to exist on the basis of tiny donations with slight incidental assistance here and there from university funds.

VI

In the meantime it is becoming more and more clear that parapsychology *does* interest—and does greatly benefit from close association with—individual scientists in a variety of adjoining fields who can, even without large research endowment, broaden the perspective of the parapsychologist, give him fruitful hypotheses and methods, and even in some instances borrow and use in their own fields concepts derived from parapsychology.

Since the interrelation of the sciences can be schematized in an infinite number of different ways, we might try our hand at a little map showing how parapsychology abuts upon several other disciplines. (1) We may conceive the anthropologists and sociologists as helping to define the social contexts in which various types of paranormal phenomena appear; the prevalent attitude toward them; and the role of such attitudes in enhancing, inhibiting, or transforming them; and in turn we may conceive parapsychologists as helping the anthropologists and sociologists to understand the biological and psychological raw materials which are molded into the religious, magical and scientific thought-forms of each period. (2) When-



ever there is deviation from "normal" functioning, as the normal is standardized by the group—whether it be epileptic visions, oracular utterances, prophetic dreams, mediumistic communications—the psychiatrists may help us to understand the dynamics and to learn in turn (as in the case of Ehrenwald and of Bendit) how the paranormal processes interact with and mold the mind and conduct of the individual. (3) The psychologists (as Schmeidler, Pratt and others have shown), and standing behind them, (4) the biologists, may help to make clear how the most general laws of normal organic functioning express themselves through the paranormal; while (5) the physical scientists—and the philosophers—with their well-formulated conceptions of time and space, may help us to perceive when and how the paranormal requires the formulation of new time-space conceptions, and to see how the events which we call paranormal fit into the general structure of the universe; parapsychology is obviously already causing some earnest efforts among physicists and physical philosophers (Strömberg, Reiser) to find new formulations.

They should all be welcomed; we need them all and they need us. It will take a heroic reconstruction in science to find an orderly place for parapsychology; and parapsychology as it becomes established will transform into new unity the whole system of the sciences. For "foreign bodies" cannot be tolerated; the recognition of the new will transform the old.

## References


1. Carington, W. *Thought Transference.* New York: Creative Age Press, 1946.
2. Crookes, W. *Researches on the Phenomena of Spiritualism.* No date.
3. Gurney, E., Myers, F. W. H., and Podmore, F. *Phantasms of the Living.* 2 vols., London: Trubner and Co., 1886.
4. Hodgson, R. A further record of observations on certain phenomena of trance, *Proc. Soc. psych. Res., London,* 1898, **13,** 284-382.
5. Humphrey, B. M. Success in ESP as related to form of response drawings. I. Clairvoyance experiments. *J. Parapsychol.,* 1946, **10,** 78-106.
6. Myers, F. W. H. *Human Personality and Its Survival of Bodily Death.* 2 vols., New York: Longmans, Green, and Co., 1903.

7. REEVES, M. P. A topological approach to parapsychology. *J. Amer. Soc. psych. Res.*, 1944, **38,** 72-82.
8. RHINE, J. B. *Extrasensory Perception.* Boston: Bruce Humphries, 1934.
9. ———. Terminal salience in ESP performance. *J. Parapsychol.*, 1941, **5,** 183-244.
10. RHINE, J. B., AND HUMPHREY, B. M. Special evidence from hit patterns: I. Quarter distributions of the page. *J. Parapsychol.*, 1944, **8,** 18-60.
11. RHINE, J. B., PRATT, J. G., STUART, C. E., SMITH, B. M., AND GREENWOOD, J. A. *Extrasensory Perception After Sixty Years.* New York: Henry Holt, 1940.
12. SCHMEIDLER, G. R. Rorschach variables in relation to ESP. *J. Amer. Soc. psych. Res.*, 1947, **41,** 35-64.
13. STUART, C. E. An interest inventory relation to ESP scores. *J. Parapsychol.*, 1946, **10,** 154-61.
14. VERRALL, MRS. A. W. Notes on the trance phenomena of Mrs. Thompson. *Proc. Soc. psych. Res.*, London, 1902, **17,** 164-244.

*Department of Psychology*
*College of the City of New York*
*New York, N. Y.*

# GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

CALL v.: To attempt to identify by ESP a stimulus object or a mental state of an agent.

CALL n.: The response described above; also the resulting selection.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI-SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards, five of each suit.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which $n$ is the number of single trials, $p$ the probability of success per trial, and $q$ the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where $\sigma_s$ is the SD of a single run and $R_1$ and $R_2$ are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a deviation in the expected direction such that the critical ratio is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.